MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1854, TO JUNE 1855.

VOL. XV.

G. B. Airy, Esq. President, in the Chair.

Wadham Lock Sutton, Esq., Berkhampstead, Herts,
was balloted for and duly elected a Fellow of the Society.

Vol. xxiii. of the *Memoirs* has recently been published. It contains three plates of engravings, besides a woodcut illustration accompanying one of the papers. The price of the volume is 4*s.* to Fellows of the Society and 8*s.* to the public. Vol. xiv. of the *Monthly Notices*, which contains shorter communications, abstracts of papers, lectures, &c., is given gratuitously to purchasers of the quarto volume. The two publications are supplementary to each other, and are to be considered as parts of the same series. They contain scarcely anything in common, except the Annual Report; and between them include a complete account of the proceedings of the Society during the year.

On the Construction of New Lunar Tables, and on some points in the Lunar Theory depending on the Conformation of the Moon with respect to its Centre of Gravity. By Professor Hansen.

(*Letter to the Astronomer Royal.*)

" As regards the Lunar Tables, I am now so far advanced with their construction as to be in possession of tables which represent the individual observations of the moon with a great degree of accuracy, and leave hardly any greater outstanding differences than those which are usually met with in observations of the fixed stars. After the correction of the originally assumed elliptic elements and of those coefficients of perturbation, the values of which can be ascertained *only* by observation, the following Greenwich observations were compared with the tables. In this operation there was no selection of the observations, those actually employed having been arbitrarily assumed beforehand, without knowing what might be the result of the comparison. Moreover, no further comparison of Greenwich observations with the tables has been instituted *subsequently* to the correction of the elements, so that the following results are unaccompanied by any circumstance which might tend to produce a bias in their favour.

The outstanding differences between the observations and the tables (theory—observation) are the following, in which a denotes the right ascension and δ the declination.

Date.	$\Delta a \cos \delta$.	Remarks.		$\Delta \delta$.	Remarks.
1824, Mar. 8	0·0			+ 1·1	
9	—2·9	...	...	—0·6	
12	+0·2	...	...	+ 6·8	
13	+2·1	...	...	—0·8	
14	—0·9	...	...	+ 5·8	
16	+2·1	..	...	—1·7	
19	—0·6	...	...	—0·7	
Aug. 31	+4·7	...	...	+0·8	
Sept. 1	+0·5	...	...	—2·5	
2	+0·7	...	...	—0·6	
4	—3·2	...	...	+2·4	
5	—1·5	...	...	...	
6	—3·5	...	...	—1·5	
8	—3·5			} —5·2	
8	—3·9	...	...		
12	+ 5·4	...	...	+4·6	
13	+2·7	...	...	+ 1·5	
15	+0·7	...	...	+0·4	
1832, Mar. 8	—6·0	...	...	+ 1·5	Faint.
9	—3·1	...	...	+ 2·0	
10	—3·5	...	...	—0·5	
11	—3·4	...	...	—0·6	
12	—0·3	...	...	+2·2	
13	—3·0	...	...	—0·2	
15	—4·2	...	...	—0·1	
17	—1·6			+0·9	
18	+2·2	F. S.	...	+2·0	
April 7	—2·3	...	...	...	
8	—1·5	...	...	—1·5	
9	+1·7	...	...	—0·6	
10	—2·2	...	...	+0·6	
11	—2·1	...	...	+ 1·7	
12	+2·0	...	...	+0·9	
13	+0·6	...	...	—0·4	
14	—1·1	F. S.		+ 5·2	
16	+2·6	...	...	—0·6	
17	+3·4	F. S.	...	—3·0	
20	+0·5	F. S.	...	—0·9	
July 4	—3·2	...	...	—1·2	
8	+3·8	...	...	+0·9	
12	+6·3	R. ...	...	+2·6	
14	+5·2	R. ...	...	+0·7	
16	+5·6	...	...	+ 5·0	
18	+ 2·4	...	...	...	
19	(+7·3)	Extr. faint. ...		—3·1	
20	(+ 10·4)	R. No star of compar.		+ 3·1	

Pond's Polar Distances of 1832 have been all increased by 2″·2.

Date.	Δα cos δ.	Remarks.	Δλ.	Remarks.
1838, May 2	−2·5	Very cloudy ...	+0·8	
3	+0·1	... ...	+2·0	
4	−1·5	... ...	−0·7	
5	+0·4	... ...	−1·5	
6	0·0	... ...	−1·0	
7	+0·9	... ...	+0·3	
8	−0·6	... ...	−2·6	
8	...	... ...	+1·2	
11	−2·9	... ...	−0·3	
12	−3·7	Cloudy ...	+4·3	Cloudy.
14	−2·2	... ...	+2·3	Faint.
15	+1·0	... ...	+3·6	
17	+4·4	No star of comparison.	+4·1	Very faint and flickering.
27	0·0	... ...	−0·1	
29	+0·6	... ...	−1·6	
31	−0·5	... ...	−0·8	
June 2	−3·7	... ...	+1·4	
4	−2·3	... ...	+1·4	
6	−2·8	... ...	+3·2	North not so good as south limb.
6	...	... ...	−0·5	
7	−0·3	... ...	−3·3	
8	−5·1	... ...	+0·3	
13	−1·3	... ...	+3·9	
1843, Aug. 4	−3·4	... ..	+2·8	
5	−2·3	... ...	−2·4	
6	−4·2	... ...	−1·4	
6	...	... ...	+2·4	
7	−3·1	... ...	+1·7	
7	...	... ...	+0·8	
9	−2·3	... ...	+0·8	
9	−0·6	... ...	...	
10	0·0	... ...	+0·5	
11	−1·7	... ...	+0·2	
12	−2·1	... ...	+2·1	
18	−0·5	... ...	−2·1	
20	−1·6	... ...	...	
22	−2·8	... ...	...	
31	−0·5	... ..	−3·2	Merely approximative.
Sept. 2	+0·5	... ...	+0·9	
2	...	... ...	+2·8	
3	+0·6	... ...	+2·5	
3	...	... ...	+2·6	
5	−1·8*	... ...	+1·1	
6	−3·5*	... ...	−1·1	
6	...	* Irregularity in the	−2·3	
7	(−8·7)*	azimuthal errors.	−2·8	
7	...	... ...	−1·9	
10	+2·1*	... ...	+0·8	

Date.	Δα cos δ.	Remarks.	Δδ.	Remarks.
1843, Sept. 11	−1·1	... ...	+0·7	
12	+1·4	... ...	+1·2	
16	−0·6	... ...	−0·9	
29	+0·6	... ...	−0·1	
29	...	... ...	−2·2	
Oct. 1	−2·0	... ...	+1·1	
4	−1·8	... ...	−1·1	
5	−1·5	... ...	+0·9	
9	+6·0	... ...	−1·2	
12	...	... ...	−1·0	
14	+1·8	... ...	−2·0	
14	...	... ...	−1·5	
15	+3·4	... ...	−1·7	
16	−0·6	... ...	−2·6	
17	+1·3	... ...	−2·2	
18	...	... ...	−1·2	
1844, Mar. 26	−5·4	... ...	{ −1·6	Both observations made w
26	...	... ...	−6·3 }	great difficulty on accou
28	−2·4	... ...	−2·9	of clouds.
29	−2·1	... ...	−2·1	
30	−0·9	... ...	−0·2·	
April 1	−0·9	... ...	+1·1	
1	...	... ...	+1·6	
2	−1·2	... ...	+0·5	
2	...	... ...	+2·5	
3	0·0	... ...	−0·5	
5	−2·4	... ...	+0·4	
6	+0·3	... ...	−2·6	
8	+2·3	... ...	+0·1	
8	...	... ...	−1·4	
9	+0·5	... ...	+2·9	
10	+0·1	... ...	+0·7	
1850, Mar. 17	−4·5	Very faint.	...	
18	−2·5	Very cloudy.	−6·0	Very faint.
25	−5·3	... ...	−0·5	
26	−4·4	... ...	+0·5	
27	−2·0	... ...	+2·6	
27	−1·7	... ...	−1·3	
28	−0·7	... ...	+3·2	
28	...	... ...	+0·8	
29	−3·1	... ...	+3·0	
29	...	... ...	+1·5	
April 2	+3·4	... ...	+0·8	
2	...	... ...	+2·3	
Aug. 13	−0·9	... ...	−1·8	
15	−0·6	... ...	+2·6	
16	...	... ...	+0·5	
18	−1·2	... ...	−1·8	

Date.	$\Delta \alpha \cos \delta$.	Remarks.		$\Delta \delta$.	Remarks.
1850, Aug. 18	...	...	...	−0·8	
19	+0·8	...	...	−1·9	
19	...	...	...	+3·5	
20	+4·3	...	...	+1·1	
21	+5·3	...	...	+1·9	
21	...	...	...	+0·7	
22	+2·6	...	...	+0·7	
22	+2·3	...	...	+1·9	
23	+2·8	...	...	−0·3	
23	...	...	...	+0·4	
24	+3·2	...	...	+0·6	
26	−3·0	...	...	−0·5	
28	+2·7	...	...	−0·4	
29	−1·0	...	...	−0·8	
30	−2·3	...	...	−1·5	
Sept. 11	+2·5	...	...	+3·4	
12	−0·2	...	...	−0·2	
14	+0·1	...	...	+0·6	
14	...	...	...	+1·0	
18	−0·2	...	...	...	
21	+1·3	...	...	+0·8	
21	...	...	...	+0·2	
22	+1·6	...	...	−2·2	
25	−0·8	...	...	−0·5	
26	−1·4	...	...	−1·9	
27	−2·6	...	...	−3·0	

Remarks upon the observations of right ascension for the year 1832.

' In the early part of August the pivots of the transit were examined and found to be unequally, &c.

' In the reduction no notice is taken of any instrumental errors, &c.

' The personal equation between Taylor and Ellis $= 0^s\cdot003$, which may be neglected.

" Nothing is stated respecting the observations F S and R. In these also the personal equation would seem to have been neglected.

' July 20, faint. The observer (12) could not observe the moon-culminating stars, the atmosphere being hazy.

" If we now exclude from the foregoing observations those which I have enclosed in parentheses, since they are obviously affected with errors, and calculate the sum of the squares of the outstanding deviations from the tables, we have,—

In right ascension,

Out of 139 observations the sum of the squares of the errors $= 952\cdot1$

In declination,

Out of 157   —   —   — $= 690\cdot4$

Whence

The mean error of right ascension $= 2\cdot62$

—   —   declination $= 2\cdot10$

"If, now, in consequence of the foregoing remarks, we also ex-
clude the observations of right ascension for the year 1832, there
results in right ascension,

Out of 113 observations the sum of the squares of the errors $= 676''\cdot2$;

consequently, the mean error of right ascension $= 2''\cdot44$.

" These errors do not differ essentially from those which usually
affect observations of the fixed stars. I may add, moreover, that
it is stated, with respect to Burckhardt's Tables, that 167 ob-
servations give the sum of the squares of the errors of the tables
$= 4602''$, which corresponds to a mean error $= 5''\cdot25$.

" Besides the foregoing observations, a number of the first ob-
servations of Bradley were compared after the correction of the
elements, whence there resulted the following differences between
the observations and the tables :—

Date. 1751.	$\Delta\alpha\cos\delta.$	$\Delta\delta.$	Date. 1752.	$\Delta\alpha\cos\delta.$	$\Delta\delta.$
Oct. 24	$+1\cdot4$	$-1\cdot5$	Mar. 2	$-1\cdot8$	$+4\cdot0$
25	$+3\cdot5$	$-5\cdot2$	4	$-1\cdot5$	$+3\cdot8$
26	$+1\cdot4$	$+3\cdot0$	6	$-6\cdot6$	$+6\cdot1$
28	$-0\cdot3$	$+9\cdot2$	7	$-3\cdot7$	$+3\cdot4$
29	$-0\cdot4$	$-0\cdot3$	7	...	$+2\cdot1$
30	$-4\cdot3$	$-0\cdot2$	9	$+0\cdot7$	$+3\cdot7$
31	$-1\cdot4$	$+1\cdot2$	11	$+3\cdot7$	...
Nov. 1	$-1\cdot3$	$-0\cdot2$	Oct. 11	$+4\cdot6$	...
2	$+7\cdot3$	$-4\cdot0$	12	$-3\cdot5$	$+11\cdot2$
2	...	$+0\cdot7$	13	$-2\cdot7$	$-6\cdot0$
3	$+7\cdot8$	$+12\cdot1$	16	$-6\cdot3$	$+2\cdot4$
7	$+1\cdot6$	$-7\cdot0$	17	$0\cdot0$	$+1\cdot2$
8	$+3\cdot8$	$-3\cdot6$	18	$-2\cdot5$	$-1\cdot8$
9	$-4\cdot5$	$+3\cdot0$	20	$0\cdot0$	$-5\cdot6$
10	$+1\cdot9$	$-1\cdot7$	21	$+7\cdot9$	$-10\cdot7$
12	$-4\cdot1$	...	23	$-0\cdot5$	$-8\cdot4$
13	$+3\cdot5$	...	24	$-4\cdot3$	$-8\cdot4$
14	$-1\cdot8$	...	25	$-0\cdot2$	$-7\cdot7$
1752. Feb. 22	$-3\cdot9$	$+5\cdot0$	27	$+5\cdot3$	$-4\cdot4$
23	$-7\cdot5$	$+2\cdot5$	28	$0\cdot0$	$+2\cdot4$
25	$+0\cdot2$	$+3\cdot9$	29	$+0\cdot9$	$+1\cdot8$
26	$+0\cdot2$	$+5\cdot9$	30	$+7\cdot7$	$-9\cdot1$
27	$+1\cdot7$	$+5\cdot3$	31	$-2\cdot4$	$-4\cdot7$
28	$+5\cdot0$	$+7\cdot7$	Nov. 1	$+2\cdot6$	...
Mar. 1	$-1\cdot2$	$+3\cdot8$	2	$+2\cdot2$	...

Date.	Δα cos δ.	Δδ.	Date.	Δα cos δ.	Δδ.
1753.	′	′	1753.	′	″
May 6	+0·2	+11·9	May 15	+6·9	+ 5·7
7	(−10·5)	+12·9	16	−0·6	+ 6·6
8	−2·3	...	22	+3·1	+ 0·4
9	−1·5	+ 5·8	23	+0·3	+ 0·1
10	−0·5	+ 6·3	24	+0·1	− 3·2
11	+1·7	− 1·1	25	+5·2	− 4·0
12	+7·3	+ 5·0	26	+3·7	− 4·4
14	+6·7	+ 4·3			

"Excluding some observations of right ascension enclosed in parentheses, we hence deduce :—

Out of 62 observations of right ascension, the sum of the squares
            of the errors .... = 884″·3
Out of 57 observations of declination .... = 1841″·0

whence,—

The mean error of a right ascension.. = 3″·78
— ,—   —   declination .... = 5″·70

Moreover, these mean errors are not greater than one might expect of such observations, when we take into consideration the observations of the fixed stars made at the same time, and bear in mind that Bradley then employed non-achromatic telescopes and the iron quadrant.

"I have also compared with the tables a series of Dorpat Observations of the moon, which furnish very beautiful results. But as there is a circumstance connected with them with respect to which I must ask the permission of our friend Struve, I will defer their publication for the present.

"I will now give the elements of the moon's orbit which I have obtained.

For the epoch,
            1800, January, 0·0^h M.T., Greenwich.

"That is to say, for mean Greenwich noon of the 31st of December of the year 1799, there were found,—

Mean longitude of the moon   = 335 43 26·71
Longitude of the perigee   = 225 23 53·06
Longitude of the ascending node = 33 16 31·15

"The following are the tropical mean motions in 100 Julian years :—

Of the mean longitude = 307 53 39·61
Of the perigee   —   = 109 3 2·46
Of the node   —   − 134 8 59·61

with respect to which I would also remark, that, although I have reason to suppose that these motions of the perigee and the node very nearly represent the true values, still I do not give them as

definitive results. It is besides a very difficult matter to deter-
mine these two motions accurately to a couple of seconds, since
the effects of their variation upon the moon's longitude and
latitude appear to be diminishing. I will now institute a com-
parison between these motions and those which you have found.

"From your paper, 'Corrections of the Elements of the Moon's
Orbit, &c.,' the following results are deducible :—

<div align="center">

Motion in 100 Julian Years.

		°	′	″
Of the longitude	=	307	53	21·0
Of the perigee	=	109	3	57·5
Of the node	= −134	8	14·2

</div>

and in the paper, 'On the Eclipses of *Agathocles, Thales,* and
Xerxes,' there is to be found,

<div align="center">

		°	′	″
Motion of the perigee	=	109	3	17·4
Motion of the node	= −134	8	54·6

</div>

"You see from this that the motions of the perigee and node
last given by you agree very nearly with the values which I have
found. I must, however, add this remark, that the observations
comprised between the years 1820 and 1850 bear an increase of
the value of the motion of perigee found by me, less than a dimi-
nution of the same value. By such a diminution the mean errors
found above might be still further diminished.

"Between the motions of mean longitude found by me there
exists a greater difference, but this arises from the circumstance
that I have slightly altered the coefficients of the two inequalities
of long period. The accurate determination of these two in-
equalities .by theory, is the most difficult matter which presents
itself in the theory of the moon's motion. I have on two occasions,
and by different methods, sought to determine their values, but I
have obtained results essentially different from each other. I am
now again engaged with their theoretical determination by a
method which I have simplified, and hope to bring the operation to a
definitive close. I have also applied to my tables some coefficients
which are not free from empiricisms, but which I can justify by
the circumstance that they represent the ancient as well as the
modern observations with great exactness ; and it may be expected
that they will represent the future observations equally well. It
is, moreover, an easy matter to replace these two coefficients in
the tables by others, should this be deemed necessary.

"I have found the following value of the eccentricity of the
lunar orbit to .be employed in the table for the equation of the
centre :—

$$e = 0·05490307.$$

"I have also found, for calculating the principal term of the
latitude, the following value of the inclination :—

$$I = 5° 8' 40''·21.$$

"The coefficient of the parallactic equation I found to be,—

$$125''{\cdot}705;$$

an amount exceeding any which has hitherto been assigned, and which indicates a greater value of the sun's parallax than has been deduced from the observations of the transit of *Venus*. The Greenwich observations, exclusive of any others, assign the fore-going value of the parallactic inequality, and the Dorpat observations nearly the same value. I cannot, therefore, alter it. I may remark further that I have taken into consideration, not only the coefficient of the parallactic inequality itself, but also the largest of the remaining terms depending on the sun's parallax, and that the aggregate of these terms may amount to the fifth part of the coefficient of the parallactic inequality.

" The coefficients of the inequalities of the moon, depending on the [terrestrial] ellipticity, were found to have the following values :—

In Longitude $= 7''{\cdot}624$; in Latitude $= 8''{\cdot}382$.

" Add to these the terms, which I have deduced from theory, depending on the variation of the obliquity of the ecliptic, and there result in Longitude the terms,—

$$7''{\cdot}760 \sin (\Theta + 184^\circ\ 42') + 0''{\cdot}000157\ (t - 1800) \cos (\Theta + 260^\circ\ 5') ;$$

in north latitude,—

$$8''{\cdot}764 \sin (v + 169^\circ\ 51') + 0''{\cdot}00038{\cdot}8\ (t - 1800) \cos (v + 278^\circ\ 39'),$$

where Θ signifies the supplement of the ascending node, and v the longitude of the moon in its orbit. The logarithm of the ratio of the moon's semidiameter, to the sine of the equatoreal horizontal parallax, was found to be as follows :—

For the horizontal semidiameter $= 4{\cdot}750{,}484$
For the vertical semidiameter $= 4{\cdot}75{,}0554.$

" It is known that different telescopes assign different dia-meters to the stars. Now, since at Greenwich the right ascensions and declinations have been observed with different instruments, it follows that the vertical and horizontal semidiameters of the moon must be deduced independently from those data. But even in the case wherein these two polar co-ordinates have been ob-served with one and the same instrument—for example, the meridian circle—it becomes necessary, when the observations refer to the moon, to execute this two-fold process. For we must bear in mind that the observations are made in different ways ; since the observation of declination is made by bringing the limb of the moon in contact with the horizontal wire of the instrument, or bringing it midway between two horizontal wires, while the right ascension is found by noting the times of the entrance of the moon's limb upon the vertical wires. It may very well happen, from the different modes of observing, that the hori-

zontal semidiameter may be found to be different from the vertical,
even in the case wherein both of them are equal—a supposition,
however, which has no foundation in theory.   Nay, I have found,
while engaged in determining the moon's orbit, indications that
the personal equation between two observers may be different for
the sun and moon from what it is for the stars, and that it even
betimes may be different for the preceding and following limbs of
the moon. I have not in the mean time been able to take into account
this difference in the Greenwich observations, but I feel convinced
that were I to do this, the agreement between the observations
and the tables would be still better than it is.   The difference
between the above assigned values of the horizontal and vertical
semidiameter is, moreover, very small, amounting only to 0'·15.
Besides the above-mentioned quantities, determined by observation,
which enter into my Tables of the Moon, there are two others of
which I shall speak further on, after I have given some account of
the mode of constructing the Tables, and of the method employed
in determining the elements.

"The perturbational coefficients employed in the construction of
the tables are, with the exception of the few which can only be
determined by observation, those which I have calculated from
theory.   They are, throughout, accurate to two hundredths of a
second : many of them I consider to be still more accurate.   They
already exhibit sensible differences from those of Damoiseau and
Plana : nay, many of them differ from the corresponding terms of
these tables to the extent of 2″ and 3″.[*]   I have not yet pub-
lished the perturbations of longitude and latitude, but the expres-
sion in the tables for the moon's parallax is, with a few trifling
alterations, that which I made known in No. 403 of the *Astro-
nomische Nachrichten*.   The good agreement of the tables with
the observations is a proof *à posteriori* of the accuracy
of my ˙perturbational coefficients, and of the soundness of the
groundwork upon which the tables rest.   I have applied to the
latter, not only the larger coefficients, but also the smaller ones,
which, indeed, are individually very trifling, but which, in their
aggregate, exercise a not inconsiderable influence on the moon's
place.   Moreover, I have taken into account, not only the tenths,
but also the hundredths of a second, since the latter produce a
sensible effect upon the sum of many terms, and if they were not
included, this sum, and consequently the moon's place itself, would
frequently come out sensibly erroneous by the tables.   Further,
the secular variations of the perturbational coefficients, which are
of sensible magnitude, are considered in the tables : their effect
in 100 years may amount to 3″.

"The mean motions of the perigee and node above assigned are
those which have been deduced from the observations, although
they admit of being rigorously found by theory.   But the ex-

_____

[*] The sum of the differences between Plana's coefficients and mine amounts
to 52″.

tremely accurate determination of them by theory, as their appli-
cation to ancient and all other observations demands, is an
operation of such magnitude, that it will be always preferable to
deduce their values from the observations. There will, therefore,
be found in the tables, with reference to these two quantities,
an empiricism, similarly as in the case of the two inequalities of
long period, of which mention has already been made.

" For the sake of simplicity, tables with double arguments are
employed in entering the smaller terms of perturbation, and, as in
the solar tables, these are so arranged, that, in practice, it will be
necessary to perform only one interpolation upon each of them
individually, the second interpolation being executed upon the sum
of all the terms. The peculiar form of the arguments of the lunar
perturbations imparts to these tables a greater degree of simplicity
than that which characterises the solar tables, since, in most
cases, they admit the employment of double arguments, whereby
the extent of every such table is reduced to half the dimensions
which otherwise it would be necessary to give to it.

" Since the most frequent use of lunar tables consists in this,—
that they shall serve to compute the moon's place for a series of equal,
or nearly equal, intervals of time, the arguments have been expressed,
not in degrees, but in time; and since the next application of them
consists in comparing with these results a large number of meridian
observations of the moon, the unit assumed in the arguments is
the mean time which elapses between two successive culminations
of the moon, in the same way as in the solar tables the mean day
is chosen as the unit of the argument. By this arrangement the
comparison of the observations with the tables will be very much
facilitated. In order to compare the observations of a month, we
select any whatever of the mean solar times of the observations,
and calculate the corresponding values of the arguments from
the tables. With this argument, increased or diminished by
the units, 1, 2, 3, &c., we calculate from the tables the cor-
responding places of the moon. These are available now for
all times which approach very near the times of the moon's cul-
mination, and by a process of interpolation which it is easy to
execute, since the interpolation factor is small, we may then
transfer the calculated places to those which refer to the mean
times of the meridian observations. The accuracy of the greater
part of this calculation can be verified by differences, and only in
respect to a small part of them will it be necessary to employ any
other test. By the aid of the same tables we may also calculate
an ephemeris of the moon, giving the moon's place from day to
day for the same mean times as is done in the *Nautical Almanac*
and other similar works. Still in this case the labour will be con-
siderably increased, since the arguments vary throughout each day,
not by a unit, but by fractions of a unit. I would prefer for such
an ephemeris tables in which the unit of the argument was a mean
solar day. This mode of computation is not, by any means, so
laborious.

"For determining the corrections of the elliptic elements, &c., the following process was employed.—With the exception of the mean motions, these corrections were deduced entirely from the observations of our own time, the ancient observations being employed only in determining the mean motion. The reason for this mode of procedure is, that the more recent observations are available in much greater abundance than is necessary for our inquiry; that they are more accurate than the ancient observations, and, therefore, must lead to more accurate results than the latter ; and that, even if the ancient observations were included in the investigation, the value of the final result would not be enhanced thereby, inasmuch as they are less precise than the modern observations.

"For every observation employed in this part of the inquiry the corresponding place was calculated from the tables, and this calculation was carried on to the observation itself. The right ascensions and declinations of the observed limbs were there-fore calculated, and the corresponding observations subtracted from them, whereby the influence of the errors of the assumed provisional values of the elliptic elements, &c. was brought out. Now, since by one of the foregoing preliminary deter-minations, the provisional elements were very nearly correct, it was found that the differences between the computed results and the observations were already very small. In order to diminish these as much as possible, the coefficients of the variations of the elements to be corrected were calculated for each observation, and the equations of condition formed. The number of unknown quantities in these equations was eleven; but besides, the co-efficients of the variations of the three mean motions were calcu-lated, annexed to the equations, and retained in the subsequent calculations, with the view of being enabled thereby to ascertain the influence of a variation of the provisionally assumed mean motions upon the remaining unknown quantities.

"The equations of condition were now distributed in groups according to the magnitude of the coefficients of the different unknown quantities, and all the equations of each group added together.

"For example, in determining the mean longitude of the epoch, the eccentricity, and the longitude of the perigee of the epoch, the observations for every 30° of anomaly were grouped together, whereby twelve equations were obtained; and a similar process was employed for the other unknown quantities. Hence resulted a large number of equations, which were treated by a process similar to that which the method of least squares leads to. Namely, the above equations, which have been cited by way of example, were multiplied and added; first, with the coefficients of the varia-tions of the mean longitude, then with the coefficients of the varia-tions of the eccentricity; and, finally, with the coefficients of the variations of the perigee. Hence resulted three final equations; and the other equations, treated in a similar way, added eight

more to these; so that the number of equations was the same as that of the unknown quantities, and the values of the latter were obtained by the solution of these equations. The final equations to which one is conducted by this process do *not* possess the property that the coefficients in the vertical rows are relatively equal to those in the horizontal rows, as is the case with the final equations to which the method of least squares leads ; but I can prove that they must necessarily possess this property *nearly*, and consequently that the cofficients in the rows referred to must be *nearly* equal to each other, since in each combination of groups all the corresponding equations of condition were employed. Nay, if even by this process the unknown quantities were not so determined that the sum of the squares of the remaining errors will be a minimum, still the result will be that this sum will approach very near to the minimum; and this will be sufficient in the present investigation, on which the rigorous employment of the method of least squares would entail a frightful amount of labour.

" I now come to a circumstance which has not yet been taken into consideration in the lunar theory. The remarkable circumstances which distinguish the motion of rotation of the moon necessarily imply a peculiar condition of the matter of which the moon's mass is composed; and theory replies in relation thereto, that the moment of inertia with respect to the principal axis, which is nearly parallel to the radius vector, must be the least; and that the moment of inertia with respect to the axis of rotation must be the greatest, of the three moments of inertia of the mass of the moon. The next assumption relative to the figure of the moon, and which is based upon this principle, is, that it is an ellipsoid, whose greatest axis is nearly parallel to the radius vector. Observation, however, has not hitherto confirmed this assumption. If, for example, this axis was to one of the other two in the proportion of 21 to 20, then upon this ground, and in virtue of the libration, there ought to be perceptible in the moon's semidiameter a variation of 2″. I have been unable, however, to establish any variation of this kind from the observations of the moon's diameter, which are available; and if even, in fact, a variation of the axis of the moon did exist, it must be much less than that deduced from the proportion above stated. Under these circumstances, it only remains that we assume that the interior of the moon is heterogeneous; and hence results the difference of the moments of inertia, which complicates the theory of the moon's rotation. There next arises, in connexion with this circumstance, the question whether, as in the case of the planets, the centre of the moon's figure coincides with the centre of gravity, as has been always assumed, or whether the positions of these two points may not be different ? If the latter condition should really exist, then there may be found many laws of the density of the interior, in virtue of which the moment of inertia relative to the radius vector which is nearly parallel to the principal axis, will be the least of the

three, even if the figure of the moon were different from that of
a sphere.

" While engaged in the determination of the elements of the
moon's orbit, of which mention has been made above, I undertook
to examine this question, and I have ascertained as·the result of
my inquiry, that the observations of the moon entirely concur in
indicating a difference between the positions of the two points
above referred to.

" Let α, β, γ, be the three co-ordinates of the centre of the
moon relative to its centre of gravity, so that α is nearly
parallel to the radius rector, β perpendicular to it in the equator,
and γ perpendicular to both of these ; then theory shows that
α and γ may be very well determined by the observations ;
but β, on the contrary, can be determined only with great
difficulty, or not at all. The coefficient of β is literally down
to small quantities equal to the coefficient of the variation of
the longitude of the perigee, and the effect of β consists, there-
fore in great part, only in this, that the longitude of the perigee
is increased or diminished by a constant quantity. For γ the
Greenwich observations give $-1''\cdot01$: and this quantity has been
determined by itself with great certainty; only I must here
remark a circumstance, which possibly may exercise an influence
upon the magnitude of these quantities. It is known, that not-
withstanding all the care bestowed in clearing the declinations
of the stars from instrumental errors, still very frequently the
declinations determined at one observatory deviate from those
determined at another observatory in one direction, either north
or south ; and it is clear that an error of declination of this kind,
should it be present, would affect the determination of γ; and
the numerical value obtained for that quantity is fundamentally
the aggregate of the above-mentioned co-ordinate γ and the con-
stant error of declination.

" I must leave undecided the question, whether in the case of
the Greenwich observations such an error may be supposed to
exist, and leave this matter to your own judgment, since, as
Director of the Greenwich Observatory, you are best acquainted
with everything relative to the observations made there ; but I ·
could not refrain from a general allusion to the circumstance ;
nay, I even felt a desire to mention it to you.

" The co-ordinate α admits of being determined by the libration
of the moon, and I have arrived at the following remarkable
theorem relative to it :—

: " *If the centre of gravity of the moon and the centre of its
figure do not coincide, then must all the coefficients of pertur-
bation for the mean longitude be multiplied by a constant factor,
which is a function of the distance between these two centres
projected upon the radius vector. If the centre of the moon be
farther removed from us than the centre of gravity, then is this
factor greater than unity; but if, on the contrary, the former*

be nearer to us than the latter, the factor will be greater than unity.

" The observations enable us to decide that this factor is greater than unity, and that, consequently, the centre of the moon's figure is different from its centre of gravity, and lies nearer to us than the latter. I have given myself much trouble in applying the greatest possible certainty to this determination; I have executed it in many different ways; and on each occasion I have found, from the Dorpat as well as from the Greenwich Observations, that this factor is greater than unity, even although its absolute value, which could not be otherwise expected, exhibited a slight difference as it resulted from the various determinations. The final determination gave this factor—

$$= 1 \cdot 0001544;$$

and hence there results, among other consequences, an enlargement of the coefficient of the evection equal to $0'' \cdot 69$, and it is easily recognised that the enlargement of the sum of all the perturbations may exceed $1''$.

" Esteemed Friend and Colleague! you have found by your discussion of the Greenwich Observations, extending from 1750 to 1830, that the principal coefficients of the lunar perturbations ought to be increased. You have found the increase of Plana's coefficient of the evection to be $= 1'' \cdot 28$, and the increase of his coefficient of the variation to be $= 0'' \cdot 68$; now, since the coefficient of the evection is almost double that of the variation, these enlargements seem to indicate the presence of a constant error.

" The increase of these coefficients which you have found is in each case greater than that which I have deduced, only I must here remark that Plana's coefficient of the evection is $0'' \cdot 34$ less than mine; and this circumstance leads to a closer agreement of our results. I would remark further, that you have found that Plana's coefficient of the annual equation ought to be increased by $1'' \cdot 07$, but your coefficient is too small by $1'' \cdot 1$, and therefore the extent of the increase will be considerably diminished.

" Permit me, in conclusion, to offer a few remarks on the foregoing explanation of the enlargement of the coefficients of the lunar perturbations. From the above-cited value of the factor it follows, that the centre of the moon's figure lies about 59,000 mètres; that is, about 8 geographical miles (reckoning 15 miles to a degree of the equator) nearer to us than the centre of gravity; and hence it follows, that between the two hemispheres of the moon, of which the one is directed towards the earth and the other is turned away in the opposite direction, there must exist a considerable difference with respect to level, climate, and all other circumstances depending thereon. Since the strata of homogeneous density must arrange themselves nearly in relation to the centre of gravity, it follows, if we suppose the figure of

the moon to be a sphere, that the centre of the visible disk of
the moon lies about 59,000 mètres above the mean level, and the
centre of the opposite hemisphere almost as much under the same
level : I say 'almost,' since if, as we here must assume, the
opposite hemisphere of the moon is more dense than the hemi-
sphere turned towards us, it necessarily follows that the mean
level of the former will be somewhat depressed, and the mean
level of the latter somewhat elevated. If we suppose the moon
to be an ellipsoid, the elongation of which lies in the direc-
tion of the earth, then the hemisphere of the moon which is
next the earth will rise a little more above the mean level,
and the opposite hemisphere will sink a little more beneath it.
Nay, we may consider it as not impossible that the surface of
the opposite hemisphere of the moon wholly or partially ac-
commodates itself to one and the same level, in a similar
way as we find to be the case with the earth.

"We need not, then, under these circumstances, wonder that the
moon, when viewed from the earth, appears to be a barren region de-
prived of an atmosphere, and of all animal and vegetable life, since,
if there existed upon the earth a mountain proportionally high, and,
consequently, having an elevation of 216,000 mètres, or 29 geo-
graphical miles, there would not be recognisable upon its summit
the slightest trace of an atmosphere, or of anything depending
thereon. We must not, however, conclude that, on the opposite
hemisphere of the moon, the same relations exist; but rather,
we should expect, in consequence of the distance of the centre
of figure from the centre of gravity, that an atmosphere and
animal and vegetable life may there find place. Nearly at the
moon's limbs the mean level must exist; consequently, we might
reasonably expect to discover there some trace of an atmosphere.

"If we now inquire into the cause of this condition of the
moon, I hold it to be not impossible that volcanic and other
similar forces in the interior of that body may have met with
far less opposition in one of the hemispheres than in the other,
and may therefore have effected much greater upheavings of
the surface in the former than in the latter. I am also disposed
to think that the phenomena termed 'Rills,' which are percep-
tible on the moon's surface, and respecting which selenographers
do not seem to have yet, upon the whole, arrived at any satis-
factory conclusion, are rents or splits, which have been occasioned
by these enormous upheavings. I submit to the judgment of
astronomers these considerations, which, strictly speaking, do not
belong to the theory to which this letter is devoted, but which
originate in the distance of the centre of gravity of the moon from
its centre of figure.

"The theory of the moon's figure, which leads to the theorem
above cited, as well as to several other conclusions, has been
developed by me in a Memoir which I have the honour of here-
with communicating to the Royal Astronomical Society.

"*Gotha,* 1854, *Nov.* 3."

Account of the Re-mounting of his Observatory, with some Observations of the Satellite of Neptune. By William Lassell, Esq.

" Previous to my sending you a very small result of my astronomical labours this autumn, I may state that owing to the encroachment of buildings around my former residence of Starfield, in the vicinity of Liverpool, I have been driven two miles further into the country in the same direction, eastward of the town; and have found at least in this locality (which I consider for the present to be situated in latitude $53° 25' 28''$, and longitude west from Greenwich $11^m 38^{s.}7$), a less smoky atmosphere, and a more rural expanse of prospect; whether a less cloudy or less disturbed atmosphere, some experience will be required to determine.

" During two or three months of the summer I was closely occupied in the removal of the two circular buildings and their domes, respectively 15 and 30 feet in diameter. The smaller dome was removed entire, on a four-wheel spring wagon. The larger was separated into four parts, as an orange is quartered, by running a saw down, so as to remove the boarding between two adjacent ribs $90°$ apart. Two of these quadrants, one partly placed within the other, were loaded, likewise on the same description of carriage, in a vertical position, nearly as when forming a part of the dome; and thus the whole in three journeys were safely brought to their new situation.

" Before the separation of the dome into quarters, iron plates, secured by screw-bolts, were attached across the intended sections of the base ring and of the semicircular arches which contain the grooves in which the shutters slide; and thus, when re-erected here, the replacement of the plates and bolts insured the exact juxta-position of the several quadrants. This plan has been attended with complete success, and the small portion of the removed boarding having been replaced and covered with new canvass, and the whole repainted, I believe no visible trace of its removal now remains.

" I embraced the opportunity thus afforded of improving the mounting of the dome, in respect of facility of motion, by turning a slight groove in the middle of the rim or circumference of the wheels, and introducing above and below a light curved railway bar. This has sensibly reduced the friction; and I now, without any appliance of mechanical power, can walk round with the dome, as it were, in my hand; thus carrying round with me a weight of 2400 lbs. without any inconvenient exercise of force.

" The stone piers and woodwork of both buildings, including the floors, wall-plates, and framing of lateral pulleys, were all removed with very slight derangement, and replaced in the two new circular buildings erected of precisely the same dimensions,— the very bricks of the old buildings being all removed for the

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purpose. The durability of this construction of dome has been in some degree tested by this removal; for, though the smaller dome has now been erected about fifteen years, no sign of decay was discovered during its translation; even the covering of canvass requiring no repair, whilst its freedom from strain or warping during the process has been such that the quadrantal shutter slides as easily and fits as well as when in its original position.

"Since the remounting of my observatory I have found opportunity for but few observations hitherto, but having been fortunate enough to obtain a remarkably good set of measures of the satellite of *Neptune* in position and distance, I request permission to put them on record by thus communicating them to the Society.

1854, August 29th, Tuesday.

Measures of Position, with powers 366 and 614.		Measures of Distance; power 614.
202	56	0·842 revolutions.
203	4	790
202	1	832
201	56	829
201	46	864
202	32	866
200	0	842
201	40	0·837
Mean 201	59	Mean 0·838
Zero 253	45	Zero —0·016
51	46	Hence 0·822 = 17″·86

38 14 = angle of position at 12ʰ 20ᵐ Gr. M.T. Mean epoch of measures.

= distance at 13ʰ 5ᵐ Gr. M.T. Mean epoch of measures.

"*Bradstones, West Derby, near Liverpool,*
9th November, 1854."

NEW PLANET.

Letter from M. Chacornac to Mr. Hind, dated Imperial Observatory of Paris, October 30th, 1854.

"I have the honour to inform you that in the night of the 28th–29th of October, at 2ʰ 10ᵐ A.M., I met with a new planet; the positions of which are as follows:—

	Paris M.T.	R.A.	Decl.	
	h m s	h m s	° ′ ″	
Oct. 28	14 17 24·3	2 33 55·16	+16 58 43·7	Equatoreal.
29	12 1 14·5	2 33 8·97	+16 55 56·9	Meridian.

"This planet is the 33d; M. Goldschmidt will have announced to you to-day his discovery of the 32d."

This planet has been named *Polyhymnia.*—ED.

NEW PLANET.

Extracts of Letters from Mr. Hermann Goldschmidt to Mr. Hind.

"*Paris, October* 3cth, 1854.

"I have the pleasure to inform you of the discovery I made in the evening of the 26th inst. of a new (32) planet. It appears as a star of the 11th magnitude. I give you the approximate positions for facilitating its research, and also those of the Imperial Observatory.

Approximate Graphical Positions.

	Paris M.T. h m	R.A. h m s	Decl. o ′
Oct. 26	10 0	2 26 13	+15 5
27	8 30	25 23	15 0
	17 20	2 25 6	+14 58

Positions of the Observatory.

Paris M.T.

Oct. 28   13^h 18^m 11^{s}·2   R.A. = R.A. $*$ -6^m 33^s·94

         ☽ = ☽ $*$ $+$ 3′ 34″·8

     Approx. Place of Star R.A. = 2^h 30^m 55^s ☽ = $+$ 14° 51′·5

Oct. 29   11^h 51^m 52^{s}·0   R.A. = R.A. $*$ $+7^m$ 1^s 80

         ☽ = ☽ $*$ $-$ 6′ 0″·2

     Approx. Place of Star, = R.A. = 2^h 16^m 33^s ☽ = $+$ 14° 44′

Oct. 29   11^h 51^m 40^{s}·1   R.A. = 2^h 23^m 33^s·03   Meridian.

"*Paris, Nonember* 3*d*, 1854.

"I certainly owe the discovery to your beautiful ecliptical charts, which have been in my possession a few months.

"Mr. Le Verrier selected the name *Pomona* for this 32d planet."

Notes on Chinese Astronomy. By John Williams, Esq.

"In presenting the Society with some illustrations of Chinese astronomy, I feel myself called upon to give some account and explanation of them.

"The first is a complete Chinese celestial atlas, in thirty-two maps. These, with one exception, have been traced from a work forming part of the Chinese Library of the late Dr. Morrison, now in the possession of University College, Gower Street, and which, by the kindness of Professor De Morgan, I have had an opportunity of examining during the past summer. This work is in 150 volumes; it is entitled 'Illustrations of the three great Principles of Nature.' These, according to the Chinese, are heaven, earth, and man. The first part of this work is devoted to astronomy. The maps have been carefully compared with those in other Chinese astro-

nomical works which have come under my observation, and also
with the Chinese star-maps in the possession of the Society, and
the positions of the several asterisms, ascertained as accurately as
possible, by collation with European maps, and by comparing
them with the 'Names of Stars and Constellations' in Dr. Mor-
rison's Chinese Dictionary, collected for that work by John Reeves,
Esq. F.R.A.S.   They have likewise been compared with a cata-
logue in a work entitled 'Observationes Mathematicæ et Physicæ
in India et China factæ a Patre Francisco Noel. Soc. Jes.,' 4to.,
Prague, 1710, which I have now the pleasure of presenting to the
Society, and also in many instances with the heavens themselves.
The results are given in the margin of each map, where will be
found the Chinese name of the star or constellation in the original
character, and also in English letters, with a reference to the
corresponding stars in European maps.   The maps are arranged
according to the Chinese custom, their order being from right to
left.

    " The second consists of a pair of celestial planispheres of large
size, published in this country about a century ago, and formerly
belonging to the Mathematical Society of Spitalfields.   On these
I have traced in red lines the corresponding Chinese asterisms,
with their names in the original characters and in English letters.
The Chinese astronomers divide the visible heavens as seen in
their country into thirty-one parts.   Twenty-eight of these may
be considered as answering to our zodiac, or, perhaps, rather to
the twenty-eight lunar mansions of the astrologers.   The other
three are called by a word signifying a wall or enclosure, and
which also implies an enclosed space.   The first of these consists
of the northern circumpolar stars.   It comprises stars in *Ursa
Minor, Draco, Cepheus, Cassiopeia, Ursa Major, Camelopardalis,
Boötes,* and *Auriga.*   The second comprises *Hercules,* part of
Ophiuchus, Serpens, Corona Borealis, and part of *Boötes.*   The
greater portion of this is bounded by a line drawn from β *Herculis*
through the head and body of *Serpens,* and the lower part of
Ophiuchus, thence through the remainder of *Serpens* to δ *Herculis.*
The third division is bounded principally by stars in *Virgo* and
Leo, and consists of part of the constellations *Virgo, Leo, Leo
Minor, Boötes, Ursa Major,* and the whole of *Coma Berenices;* I
have distinguished the names of these three divisions by inscribing
them within an orange-coloured line.

    " The remaining twenty-eight divisions comprise the rest of
‧ the heavens.   They are exceedingly unequal in extent, some con-
taining many degrees of right ascension, others very few.   They
take their names from a group of stars generally situated in the
centre of each division.   The surrounding groups have various
names, and in this respect answer in some measure to our constel-
lations.   They are, however, far more numerous.   The figures of
these groups, or constellations as they may be called, have no
resemblance whatever to those given in European maps, the stars
of each asterism being connected merely by straight lines.

" It is, however, considered that each of these twenty-eight divisions or constellations has a particular animal on earth which corresponds to it, and also that it is under the influence of one of the seven heavenly bodies, which are the Sun, the Moon, and the five planets, *Mercury, Venus, Mars, Jupiter,* and *Saturn.* Some authors say that twenty-eight celebrated generals, who flourished during the Han dynasty, *i. e.* from B.C. 206 to A.D. 223, were, after death, changed into these twenty-eight constellations; thus affording a curious correspondence with the ancient astronomy of the western world, in which celebrated characters are fabled to have been changed after death into constellations. The Chinese also parcel out these twenty-eight divisions into four portions of seven each. According to them, the first seven are to be reckoned from the south towards the east; the stars from which they take their names are situated in *Virgo, Libra, Scorpio,* and *Sagittarius.* The next seven are between the east and north, consisting of stars in *Sagittarius, Capricornus, Aquarius, Pegasus,* and *Andromeda.* Seven are situated between the north and west, comprising stars in *Andromeda, Aries, Musca, Taurus,* and *Orion;* and the remaining seven, between the west and south, being stars in *Gemini, Cancer, Hydra, Crater,* and *Corvus.* It must, however, be observed, that these divisions are not only very unequal in extent from east to west, but also from north to south, and that they differ exceedingly in the comparative number of stars in them. Thus the division *Ke* consists only of four stars in *Sagittarius* and *Telescopium,* forming the central group giving the name, and three stars in *Ara,* while the division *Tsing* consists of the whole of *Gemini,* having to the north stars in *Perseus,* and to the south stars in *Canis Minor, Monoceros, Canis Major, Argo,* and *Columba.* Again, the division *Tsuy* comprises the stars in the head of *Orion,* and a few to the north in *Auriga;* and the succeeding one, *Tsan,* consists of the remaining bright stars in *Orion* and stars in *Lepus, Columba,* and *Eridanus.* I instance these as examples of the extreme inequality of the several divisions.

" In these planispheres I have distinguished the names of the twenty-eight divisions by inscribing them within blue lines.

" In addition to the thirty-one divisions of the visible heavens, I have inserted, both in the planispheres and in the atlas, the southern stars not visible in China. These are taken from one of the star-maps in the possession of the Society; and I must here observe that, however ancient and original the northern asterisms of the Chinese may be considered, there is internal evidence to prove that these southern constellations, at least, have been derived from European sources. Thus the constellation *Phœnix* is called in Chinese ' Fire Bird;' *Dorado,* ' Gold Fish;' *Apis,* ' Honey Bee;' and so on ; and the stars of each constellation are also confined to the figures, as given in our maps,—a circumstance which does not occur in the more northern asterisms.

" In tracing upon these planispheres the Chinese asterisms, every care has been taken to render them as accurate as possible. The

chief authorities from which they were taken were the Chinese
star-maps in the possession of the Society. These consist of
    " Two planispheres, giving the northern and southern hem :-
        spheres. Presented by Capt. Sir E. Home, Bart. R.N.
    " The northern and southern hemispheres, in one sheet, with a
        list of the constellations and other particulars.
    " An impression from the wooden block in the possession of
        the Society, showing the stars visible in China.
    " Two planispheres in manuscript; giving the .northern and
        southern hemispheres. On these the figures of the asterisms
        are very neatly drawn, and their names very distinctly
        written. The number of the stars in each constellation is also
        given, and each star has its corresponding number.
    " A planisphere, showing the stars visible in China, and in a
        smaller circle, the southern stars not visible in the north of
        that country.
    " The whole of these were presented by Mr. Reeves.

    " The first three of these are evidently from European au-
thorities, as divisions and numerals appear on them, which could
not be derived from any other source. They are also rather
meagre in names, often in many parts ill printed, and much
confused in the details.
    " The last two are more distinct, and appear also to be more
original; and these formed the basis of the work I had undertaken,
and afforded me very valuable information and assistance in it.
Every group of stars in these was carefully examined; the relative
position of each star determined as accurately as possible, and
verified by comparison with Mr. Reeves' valuable contribution to
Dr. Morrison's Dictionary, to which I have before alluded. They
were also compared with the catalogue in Father Noel's work, also
mentioned before. This I found of great assistance in doubtful
cases. The other planispheres were also referred to occasionally;
and although I cannot be satisfied that in every instance I have
attained perfect accuracy, still I may confidently say that the
positions assigned are, generally speaking, sufficiently correct for
all ordinary purposes.
    " The work by Father Noel, to which I have more than once
alluded, is the third contribution to this subject. Among other
things, it contains an account and explanation of the Chinese
cycle of 60 years, and of the mode of its formation. It also con-
tains a catalogue of stars, or rather asterisms, arranged according
to our constellations, with the Chinese names in English characters,
spelt according to the Portuguese method.
    " In one part of the work the author regrets being compelled
to omit the original Chinese characters for the names, &c., as he
had no means of procuring the requisite types. This defect I
have supplied in manuscript; and the necessity of it will be suf-
ficiently apparent when I inform you that the Chinese vocables,
being very limited, each has, in consequence, to express the sense

of many characters, whose form and meaning are totally different. Thus the characters answering to the sound of our letter E are at the least not fewer than 267, all of them distinct in their meaning, and varying exceedingly in their forms. It, consequently, follows that any attempt to ascertain the meaning of the simple vocable E occurring in a Chinese sentence written in European letters, would be almost, if not absolutely, unavailing without the original character intended to be expressed by that sound.

" In conclusion, I have only to express my hope that the pains I have bestowed on this subject will not be entirely without their use to such persons as may hereafter have occasion to investigate Chinese astronomy. At all events, they will save a vast deal of preliminary labour, which would, of necessity, be required before the party could be in a position to proceed satisfactorily with the object he might have in view. I may also state that it is possible I may, on some future occasion (should it be agreeable to the Society), enter more at large upon Chinese astronomy; and by giving an analysis of a work on this subject, published by themselves, which is in my possession, enable you to form some idea of the value of the astronomical attainments of that singular people.

" *November* 1854."

A Method of finding the Greenwich Mean Time at Sea by Computing the Moon's true Right Ascension from the observed Lunar Distance. By M. C. Rümker.

(*Communicated by the Astronomer Royal.*)

" When the observed distance of the star from the moon is small and the difference of their declination great, the error committed in determining the Greenwich mean time, by assuming the change of time proportional to that of the distance, can scarcely be compensated by the method of taking the difference of successive proportional logarithms, whereas the change of the moon's right ascension will be found more regular. I propose, therefore, to convert the observed sun's or star's distance from the moon into her true right ascension, whence the Greenwich mean time may be found by interpolation in the *Nautical Almanac* with so much the more ease and precision, as the moon's place is given there for every hour. Thus, moreover, the expense of the calculation and printing of the distances might be saved, and the volume of the *Almanac* become less voluminous.

" It remains, therefore, to be shown that the moon's true right ascension may be found with no less accuracy, and by a not more complicated calculation than the true distance.

" In order to determine the longitude, the mean time on board must be ascertained, whence the hour-angle of the star is known, and by subtracting this angle from their difference of right ascension, or adding it thereto, we obtain the hour-angle of the moon. Denoting now by a the difference of the true right ascensions of

the sun or star and the moon, by D, their apparent distance, by
p, p', their true, and by P, P', their apparent polar distance ; by t
and $t' = \alpha \mp t$, their hour-angles ; by z, z', their zenith distances ;
by ι, ι', their parallactic angles ; by $\varrho - \pi$, or $\pi' - \varrho'$, the differences
of refraction and parallax in altitude ; by $d\alpha$ and dp, the effect
thereof on the right ascension and polar distance ; by φ', the geo-
centric latitude of the place of observation, lest at sea it might be
judged sufficient to assume the geographic latitude for it ; then the
strict formula, which answers to both bodies by substituting p' for
p, ι' for ι, and $\pi' - \varrho'$ for $\varrho - \pi$, when applied to the moon, is

$$\frac{\sin t \cos \varphi'}{\sin z} = \sin \iota, \quad \tan (\varrho - \pi) \cos \iota = \tan \pi, \quad \frac{\sin n \tan \iota}{\sin (p \mp n)} = \tan d\alpha,$$

$$\tan \frac{(p \mp n)}{\cos d\alpha} = \tan P.$$

" The lower sign applies to the moon ; but if ι is obtuse the
signs must be changed.
" Generally it will be found sufficient to make

$$(\varrho - \pi) \cos \iota' = dp \quad \text{and} \quad \frac{(\varrho - \pi) \sin \iota'}{\sin (p + P)} = d\alpha,$$

where the following practical rule may serve as guide or con-
firmation.
" When the parallactic angle ι' is acute, then is

True Polar Distance $\begin{cases} p \text{ of the sun or star} - dp &= \text{apparent polar distance P} \\ p' \text{ of the moon} + dp' &= \text{apparent polar distance P'.} \end{cases}$

" When the parallactic angle is obtuse the signs must be
changed. Should any doubt exist whether ι is acute or obtuse,
it must be found by another, to be shown hereafter. Then is,
when A denotes the apparent difference of right ascensions,

$$\sqrt{\frac{\sin \frac{1}{2} (D + P - P') \cos \frac{1}{2} (D + P - P')}{\sin P \sin P'}} = \sin \frac{1}{2} A.$$

" If, now, the sun or star, and the moon, were observed on
different sides of the meridian, then is

$$A + d\alpha - d\alpha = \alpha ;$$

but if both bodies were observed on the same side, then is,

When the sun or star was highest   A $- d\alpha - d\alpha'$
moon was highest   A $+ d\alpha + d\alpha$,

and the sun's or star's right ascension $\pm \alpha$ = moon's right
ascension.
" In the following example the reduction to the geocentric
zenith has been neglected.
" Sept. 30, 1857, at 11^h 15^m 54^s mean time P.M., being then in
16° 29' south latitude and 92° 2' west longitude, a set of distances

of *Jupiter* from the moon's remote limb, with the corresponding altitudes, were observed from 22½ feet elevation above the level of the sea, the height of the barometer being 29^{in}·6 ; height of thermometer 14° Réaumur. The chronometer was 2^m 9^s slow of Greenwich mean time. The means gave

Chronometer.	Jupiter, East.	Moon.	Dist. of remote Limb.
	° ′ ″	° ′ ″	° ′ ″
21 54 A.M., 31 Sept., Gr.	36 32 0	68 53 37	69 26 18·2
1 9	Dip —4 42	— 4 42	Ang. Sem. — 16 16·7
24 3 Gr. M.T.	Ap. Alt. 36 27 18	68 48 55	D 69 10 1·5
	☽ — ☿ — 1 15·2	Moon's Ang. Sem. 16 17	
	True Alt. 36 26 3	Ap. Alt. 69 5 12	
	Z = True Zen. Dist. 53 33 57	☿ — ☽ + 20 34·7	
		True Alt. 69 25 46·7	
		Z′ 20 34 13	

" For the Greenwich mean time we find in the *Nautical Almanac* the sun's mean R.A. = 12^h 39^m 34^s·6 ; the moon's Declin. — 11° 8′ 45″; eq. hor. par. 58′ 37″·7; Red. — 1; the moon's semid. 16′ 0″·7 ; *Jupiter's* R.A. 2^h 51^m 8^s·6 ; Decl. + 14° 59′ 0″; hor. par. 2″·0 ; the moon's app. R.A. 22^h 33^m 34^s·6.

" The hour-angle *t* of *Jupiter*, computed from his altitude results, 2^h 55^m 40^s·1, which, being subtracted from their difference of R.A. 4^h 17^m 34^s·0, leaves for the moon's hour-angle *t′* = 1^h 21^m 53^s·9. The parallactic angle of *Jupiter* is found, ϛ = 55° 45′ 34″, *dα* = 64″·35, P = 104° 58′ 17″·5. For the moon we obtain *s′* = 72° 40′ 26″, *dα* = 20′ 0″·9, P′ = 78° 57′ 22″·8,

A	64 42 18			
dα	+ 1 4·35			
dα′	— 20 1·1			
	64 23 21·25			
ϛ in time	4 17 33·41			
Jupiter's R.A.	2 51 8·60		3600	Log 3·556303
Comput. R.A. of Moon	22 33 35·19		0 50·82	log 1·706035
Moon's R.A. at 17^h	22 32 44·37	Diff.		log C 7·807052
18^h	22 34 51·12	Diff. 2 6·75		3·159390
Green. M.T.	17 24 3·4			
M.T. on board	11 15 54·0			
Long. in Time	6 8 9·4 = 9° 2′ 21″ West.			

" It is true that an error in the polar distances will affect the calculation of the difference of right ascension, under the above-mentioned supposition of a small distance and a proportionally great difference of declination, more than under more favourable circumstances. But it is equally evident that a *tabular* error of declination will influence, in the same measure, the true distance in the *Nautical Almanac*, which, in that case, will be affected with nearly the whole error of the declination, whereas the influence of

any faults in the refraction and parallax, arising from erroneously observed altitudes, will remain the same, whether $\varrho - \pi$ is divided in its effects upon the distance, or upon right ascension and polar distance. Indeed, we may say the same as to the clearing of the apparent distance, when the difference of azimuths is small, and the difference of altitudes great, where then the true distance will be affected with nearly the entire errors of refraction and parallax in altitude. Hence it follows that, with respect to accuracy, there is no other difference between the two methods, than that the interpolation may be achieved more correctly by means of the right ascensions given for every hour, and that the distances may be spared. Nor offers, when the altitudes have been observed, the method proposed by me any advantages with respect to the numerical calculation. But setting aside that little dependency can be placed upon the altitudes of stars observed above the sea-horizon, on account of the variable terrestrial refraction, and that this horizon can scarcely be distinguished at night, it should be remembered that even in the day-time the altitude of the moon's crescent, when high and faint, is difficult to be observed, and that when the line joining her cusps is perpendicular to the horizon, her real upper and lower limbs are invisible ; and as it may hardly be expected that the more disinterested assistants, with their not always well-adjusted quadrants, will be sufficiently attentive to the given signal, it must be owned that, if the chronometer, or approximate Greenwich time, is but known within moderate limits, the declination of the heavenly bodies is a more certain argument than their altitudes, and that computed altitudes of the moon and stars are generally better than the observed altitudes. Not to expatiate, I will only remark, that if the latitude is sufficiently known, the mean time on board may always be obtained by altitudes of the sun observed previous or subsequent to the distance, and almost independent of the latitude, twice a-day when the sun is in the prime vertical ; so that, with the assistance of a good time-piece and the log, the mean time may be known at all times of the day, and be controlled by observations on the next day. It will, therefore, be advisable, when a lunar distance has been taken, at least to confirm or to correct the observed altitudes by computation, which offers the opportunity of introducing the geocentric latitude ; and in that case, the method of finding the Greenwich time, by the right ascension of the moon, requires less logarithms than a correct method of clearing the distance.

" The parallactic angles and the altitudes may be found by any other formula, but I propose the following :—

$$\cot \varphi' \cos t = \tan m, \quad \frac{\cot t \sin(p \mp m)}{\sin m} = \cot t', \cot (p \mp m) \cos t' = \tan h$$

where φ' is the geocentric latitude, m an auxiliary arc, h the altitude ; the lower sign must be used when the hour-angle is obtuse.

" May 29th, 1857, in latitude $53° 33'$ North, and longitude

9° 58′ 30″ East, of distances of the sun's and moon's limbs were observed, corresponding to a watch regulated to mean time. Barometer 29^m·9 Thermometer 15° Réaumur.

Mean of Times.	Mean of Distances.
h m s	h m s
4 39 54 P.M.	79 29 36·5
	15 36·5
	15 47·5
Appt. Dist. D	80 1 0·5

"From the *Nautical Almanac* we obtain sun's mean R.A. 4^h 28^m 29^s·49 ; sun's true R.A., 4^h 25^m 36^s·623 ; equat. of time, + 2^m 52^s·9 ; sun's Decl., +21° 41′ 14″·3 ; sun's semid., 15′ 48″·5 — 1′ contract; moon's R.A., 10^h 4^m 28^s·45 ; moon's Decl., + 15° 2′ 35″·8 ; moon's semid. 15′ 24″·5 + augmen. 12″; moon's eq. hor. par. = 56′ 24″·8, Red. for spheroid = 7″·2 ; geocentric latitude φ'= 53° 22′ 10″, t=70° 41′ 43″·5, t'=14° 1′ 15″.

"Thence we find for the sun i=39° 55′ 41″, Z=61° 19′ 29″ =ϱ —π by second approx.= 1′ 44″·3 ; $d\,\alpha$=1′ 12″·05 ; P=68° 17′ 26″.

"For the moon i'= 13° 1′ 40″; Z'=39° 53′ 8″·5; π'—ϱ'= 35′ 45″·5; $d\,\alpha'$=8′ 19″·48; P'=75° 32′ 14″·3; A 84° 50′ 9″·0; α=84° 43′ 1″·57, in time 5^h 38^m 52^s·105, which, being added to the sun's true R.A. 4^h 25^m 36^s·623, gives for the moon's true R.A. 10^h 4^m 28^s·728, whence the longitude will be found 9° 58′·24."

On the Connexion between the Sun's Motion and the Variations of Terrestrial Magnetism. By Professor Secchi.

(*Extract of a Letter to Capt. Manners, R.N.*)

"Colonel Sabine, as you know, has published and discussed with admirable sagacity an immense mass of observations of the phenomena of terrestrial magnetism, and in the last volume of the the Toronto Observations has shown that in the tropical regions the opposition of oscillation of the needle follows so strictly the change of the sun's declination, that the change of the former can be traced to the very day of the equinox. This consequence, although apparent from the curves of the tropical countries, was not so clearly perceived in the countries more distant from the tropics. But it was evident, that if the law was true, it would become visible everywhere, provided a suitable mode of analysis should be applied. I thought, therefore, that the want of its being apparent there, was due to the kind of graphical analysis used until now, which consists in referring the monthly curves, either to the absolute mean of the year, or to the mean of the month itself. I undertook, therefore, to refer the monthly curves to the mean annual hourly curve of the year. The reason of doing so is this : the effect of diurnal variation at any hour (as far as it depends on the sun) is the complex effect of its hour-angle and of declination. The mean annual hourly curve, as deduced from the observations of the whole year, is a residual function independent of the declination which acts in opposite directions in

the two halves of it. Therefore, if from any mean monthly curve
we subtract the mean hourly curve of the year, the residual quan-
tity so obtained will be a function of the declination of the sun. I
have, therefore, executed these operations (which are done very
easily, using the tables and plates of Colonel Sabine exhibiting the
monthly ranges of the needle variation in declination), and the
result has been a good deal more satisfactory than I ever expected.
The results for the four colonial observatories are contained in the
figures here enclosed, and a simple glance at them shows that for
opposite declinations of the sun their curvatures are directed in
opposite parts, and that their change can be traced to the very day
of the equinox. Even some irregularities which may puzzle in the
figures of Colonel Sabine for the tropical regions, have their expla-
nation here. Thus, for instance, at St. Helena the curve of June
has a descending flexure which is wanting in that of January.
Now the differential curves of the declination show that this
branch disappears from the interference of a similar branch, but
opposed in the two months of opposite declination. We can gene-
rally state, that the mean curve of the declinometer at any country
of the globe results from the interference of the two curves, one
depending only on the diurnal period, and the other on the de-
clination of the sun: the first is constant in its direction through
all the year, and the second changes according to the position of
the sun relatively to the equator. The inspection of these curves
shows a general feature of the solar periodical influence: this is,
that the maximum and minimum differ in time very nearly six hours;
and their phases depend on the passage of the sun through the
magnetic meridian of the place, so that an acceleration occurs
in those places where the north declination is greater. For the
southern hemisphere the complete antagonism of forces requires a
contrary law. .

    " These facts seem to strengthen the opinion that the solar
action is a direct one on the needle. To verify this hypothesis, I
have subjected it to calculation in this way : I consider the needle,
according to the Anpérian theory, as an element of galvanic cur-
rent circulating during the day in a circle, which is the parallel
that it describes during the diurnal rotation of the earth ; and
applying to this hypothesis the formula given by Savary for the
action of a magnet on an element of current circulating in a circle,
neglecting the terms of the order higher than the first and those
depending on the parallax of the sun, I obtain the three fol-
lowing expressions for the components of the solar action,
relatively to the horizon as a fundamental plane of co-ordinates :

$$X_0 = \frac{k \, l \, R}{r^2} \sin \delta \sin \omega,$$

$$Y_0 = \frac{k \, l \, R}{r^2} \cos (L + \delta) \cos \omega,$$

$$Z_0 = \frac{k \, l \, R}{r^2} \sin (L + \delta) \cos \omega,$$

and the resultant,

$$S = \frac{k\,l\,R}{r^2}\,\sqrt{1 - \cos^2 \delta \sin^2 \omega}.$$

Where X_o is the component directed perpendicularly to the meridian along the line east-west; Y_o that directed along the line south-north, and Z_o is vertical; k is a constant co-efficient dependent on the intensity of solar force; l, the length and magnetical moment of the needle; r, the distance of the sun from the centre of the circle; R, the radius of the parallel described by the needle; L, the latitude of the place; δ, the declination of the sun; ω, its hour-angle. If now we call Δ the deviation of the needle from its mean position, and P the resultant of those forces which act in that plane in which the needle is only moveable, we will have

$$\Delta = \frac{P}{\tau}\,\sin h + 2\,\frac{P^2}{\tau^2}\,\sin 2 h + \quad \ldots \ldots$$

where τ is the component of the force of terrestrial magnetism or gravity, which acts in the plane of P, and h the angle between P and the mean position of the needle; and also here we will retain only the first term.

"To find the different laws of motion for the three well-known magnetical instruments, let us suppose first a place where the declination of the needle $= o$: in this case the magnetic and astronomical meridians coincide; and for the declinometer we shall have $P = S \cos b$, b being the altitude of the sun, and calling a its azimuth, it will be

$$\Delta = \frac{k\,l\,R}{\tau\,r^2}\,\sqrt{1 - \cos^2 \delta \sin^2 \omega}\,\cos b \sin a,$$

and since

$$\cos x = \cos b \sin a = \sin \omega \cos \delta,$$

we have

$$\Delta = \frac{k\,l\,R}{2\,\tau\,r^2}\,\sin 2 x,$$

calling x the angle made by the sun with the first point of east. Likewise for the bifilar magnetometer we have

$$\Delta b = \frac{k\,l\,R}{\tau\,r^2}\,\sin x \cos y,$$

y being the angle between the sun and the fourth point of the horizon; that is, the angle between the sun's radius vector and the axis $o\,y$. Finally, for the vertical-force magnetometer, in the prime vertical

$$\Delta_0 = \frac{k\,l\,R}{r^2}\,\sin x \sin y \sin h', \quad \cdot$$

the angle h' being given from the equation $\tan h' = \cot b \sin a$. These formulæ give for the declinometer a periodical variation,

which is double during a day everywhere. But for the bifilar magnetometer they show that on two occasions its period, which is generally also a double, is reduced to a simple one, viz., when $L = o$ and $\delta = o$. The vertical-force magnetometer, also, has a simple period at the equator complementary to the bifilar, as appears clearly from the value of

$$P = \sqrt{X^2 + Z^2}$$

which for $L = o$ is reduced to

$$P = \frac{k\,R\,l}{r^3}\,\sin \delta,$$

which multiplied by sin h', gives a simple period. In any other place the period is always double. These conclusions agree with the Observations, and explain the march of the components of horizontal and vertical force at St. Helena; and they exhibit the simple period of the bifilar magnetometer at the equinoxes, which is sufficiently well proved from the observations. These formulæ give for the day and night equal excursions, because they make abstraction of the influence of the interposition of the terrestrial globe during the night-time. This being magnetical, diminishes strongly the solar influence. Hence, the night excursions exist, but exceedingly much diminished in extent. We can apply the same formulæ to this case also, if we suppose the distance of the needle much increased, so that the night-range should become $\frac{1}{3}$ or $\frac{1}{4}$ of the day-range, as is manifest from the figures given by Sabine, *Hob. Observ.* (vol. i. p. 48, plate.) This also may explain some irregularities in the day-period; since a period begun, when the earth is not interposed (the needle not being yet in the shadow), will be partly destroyed by the supervening discontinuity of action. This, practically, must be very often the case, since the co-ordinate axes, which have been supposed to be directed according to the fundamental points of the astronomical horizon, must be rather directed towards the cardinal points which refer to the magnetic axis of the earth, and then w must be considered and computed from the sun's passage at the magnetic meridian. Our formulæ may explain also the singular fact of irregular perturbations of the needle happening at certain fixed hours. This phenomenon, although supposed to be connected with meteorological causes, is evidently also in connexion with solar periods, so that the maxima of perturbation arrive at the hours of maxima and minima of nocturnal periods. This is the case with the nine o'clock perturbations. To explain all the phenomena of diurnal variation is certainly an attempt superior to the state of our actual knowledge of magnetism, since this explanation depends upon the law of distribution of magnetism on the earth and the sun, and upon the law of magnetical induction, both which are completely unknown to us; what I have exposed here is only to try if, by means of the known laws of nature, we can thrown any light on this difficult subject. It is obvious that I do not mean to exclude

variations of magnetism due to meteorological influences and au-
roras, or to any other cause of irregular perturbation, since I have
taken into consideration only the effects of the sun, and even
neglected some small parts and coefficients, which may become
sensible perhaps in individual cases, and explain minor periods.

"*Roman College Observatory,*
   "*Rome, Sept.* 9, 1854."

On Saturn's Rings. By J. R. Hind, Esq.

"In the *Monthly Notice* of this Society for June 1853, is given
an extract from a letter of Sig. Secchi's, addressed to Capt. Man-
ners, R.N., referring to a drawing of *Saturn's* ring, executed by
Campani at Rome in 1664, and considered to afford confirmatory
evidence of that remarkable conclusion deduced by M. Otto
Struve, — that the ring is contracting upon the ball at a rate
which at no distant period may bring it into contact with the
globe.

"I have not seen it noticed anywhere in print, that the first
volume of Lubienietski's *Theatrum Cometicum* contains a well-
engraved sketch of the appearance of *Saturn* and his ring, no
doubt founded upon the drawing, the original of which was for-
warded by Signor Secchi to Capt. Manners.* It accompanied a
letter from Father Athanasius Kirch to Lubienietski, dated Rome,
1665, July 25, and is thus alluded to in that communication,—
'*Mitto hisce inclusum Systema Saturninum à Campano inge-
niosissimo Telescoplopæo* 50 *palmorum tubo observatum.*' It ap-
pears to have been taken at the end of July 1664, when the ring
was very open. On measuring the relative breadths of the ring
and the dark space between the interior edge and the ball, the
latter is seen to be very slightly the broadest. Reducing the di-
mensions to seconds of arc with M. Otto Struve's value for the
equatoreal diameter of the globe, I find for the breadth of the
rings 5″·58 and of the dark space 5″·77. These numbers agree
very well with the relative breadth inferred from a remark of
Huyghens, in his *Systema Saturnium*, applicable to the year
1657, — that the breadth of the interval between the interior
border of the ring and the body of the planet was equal to, or
perhaps rather exceeded, the breadth of the ring. M. Otto
Struve argues that this observation of Huyghens, when taken in
comparison with recent measures, is alone sufficient to prove that
a very considerable change has occurred in the system of the rings
since the middle of the seventeenth century: Campani's drawing
is certainly rather favourable than otherwise to this view.

"The roughly executed diagram of *Saturn* and his ring, at
the time of the greatest inclination of its plane to our line of
vision in July 1667, found in Lemonnier's *Histoire Céleste,*—

* See *Monthly Notice* for June 1853.

though apparently less deserving of confidence than that given by Lubienietski, may be worth mention, as the evidence it affords, whether reliable or not, tends to support M. Otto Struve's inference. In this sketch the breadth of the dark space, compared with that of the rings, is about as 3 to 2. The diminished breadth of the space in the diagrams which follow may be accounted for by the closing up of the rings and the imperfections of telescopes in those times. It should be remarked, however, that Picard's avowed object in presenting this drawing, taken in July 1667, was to point out the extension of the ring beyond the edge of the globe, and had no reference to the comparative breadths of the ring and dark interval.

"I have found amongst Picard's observations on the ring, in the same work, a notice of that second dark belt, traversing the globe at the interior edge of the rings, which, with every appearance of probability, has been identified with the obscure ring of modern observers. M. Otto Struve alludes to observations of this nature made early in the eighteenth century; but probably Picard's had escaped his notice. Under date 1673, June 15, it is remarked, '*Saturne étoit sorti des raïons du soleil: il y avoit deux barres noires qui marquoient les deux bords intérieurs et extérieurs de l'anneau.*' A rough sketch is annexed to illustrate the observation. It appears difficult to account for this belt on any other assumption than the above; and we have, therefore, reason to suspect the existence of the obscure ring, at least as far back as 1673, notwithstanding the negative evidence which the subsequent observations of Sir W. Herschel and others may be considered to afford.

"At page 25 of the *Histoire Céleste* are some measures of the diameter of the ring by the same observer in the years 1666 and 1667, and approximate determinations of the angle of position of the greater axis with respect to the meridian. On the following page are also measures of the diameter of *Jupiter,* between October 3d, 1666, and November 10th, 1667; yet, strange to say, Picard does not appear to have remarked the compression at the poles during the interval over which these measures extend, since, in a note dated 1673, April 13, he says, 'I began to perceive that the disk of *Jupiter* was a little oval, and that the greater diameter is always in the direction of the belts.' It is uncertain, therefore, in what direction the planet was measured; but to obtain something like a probable value of the major axis of *Saturn's* ring in 1667, I have compared the measures of *Jupiter* with the mean results of Struve and others, assuming that Picard's numbers represent the *mean* diameters of the planet, and thus have deduced a correction to be applied to the measures of *Saturn.* In this way I find the exterior diameter of the ring = 44"·1 (for *Saturn's* mean distance). If we compare with the values given by the observations of diameter with the meridian instruments at the Royal Observatory, this number would be increased to 47"·1. In either case it is larger than the measures of Bessel, O. Struve, and

others, now indicate,— a mean of their results being 39″·5 at the same unit of distance. So far, then, as Picard's observations may be considered deserving of reliance, there is evidence that the exterior diameter of the ring is diminishing, with the gradual approach of its inner edge to the ball : the rate of contraction being apparently about 2″ in a hundred years. In saying thus much, however, I am well aware that great uncertainty attaches to any conclusion that we can arrive at on this subject, with the data at present before us."

Those who are interested in the curious theory respecting *Saturn's* rings, recently propounded by M. Otto Struve, and who are of opinion that the observations of the seventeenth century may be to some extent legitimately employed in testing that theory, will find an original representation of the planet and its appendage by Campani, in the MS. *Letter Book* of the Royal Society (vol. i. p. 324). This drawing, which appears on a small slip of paper carefully inserted in the volume, accompanies a letter from Auzout to Oldenburg, in which the writer states that it had been forwarded by Campani himself in a letter to Cassini, who was then residing at Paris. It is dated October 5, 1665. There are also two drawings of *Saturn*, as seen by Cassini in the years 1676 and 1677, to be found at page 584 of vol. x. of the *Hist. Anc. Acad. des Sciences*. It may be remarked that the interesting observation of Picard's respecting the spheroidal figure of *Jupiter*, which is cited by Mr. Hind in the foregoing paper, is the earliest *distinct* recognition of a real ellipticity of any of the celestial bodies which is to be found in the records of astronomical science, as the writer of this note has recently shown, it is to be hoped satisfactorily, in a paper *On the Early History of the Researches of Astronomers relative to the Spheroidal Figure of the Earth*, which is inserted in the *Monthly Notice* for June of this year (vol. xiv. p. 232), and to which the reader is referred for various other original notes and observations of a similar kind to that of the French astronomer. The remark of Picard is the more creditable to the acuteness and sagacity of that admirable astronomer, when it is considered that hitherto no other individual, with the exception of perhaps Cassini, appears to have entertained the remotest suspicion of the ellipticity of the planet, notwithstanding that it had been the object of continual scrutiny by observers since the invention of the telescope about the beginning of the seventeenth century. As regards Cassini, it may be stated, that, down to the year 1690, he had not entertained *a decided conviction* of the ellipticity of the planet, since in that year we find him still impressed with the idea that its disk was quite round, as is evident from the following passage extracted from the Registers of the Academy of Sciences of Paris for the year 1690 :—" *M. Cassini remarqua à cette occasion que Jupiter, qui lui avait parût autrefois d'une figure un peu ovale, dont le*

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plus grand diametre tendait d'orient en occident lui paraissait à present parfaitement rond."—(*Hist. Anc. Acad. des Sciences,* vol. ii. p. 108.) In the very next year he announced definitively the spheroidal figure of the planet, and assigned to the ellipticity a value which appears to be about as good as any that has since been given.—EDITOR.

First Elements and Ephemeris of Polyhymnia,
Computed by Mr. George Rümker, from the following
Observations :—

<div align="center">

Paris,   Oct. 29, 1854.
Berlin,   Nov. 3,
Durham, Nov. 9,

</div>

	° ′ ″	
Mean Anomaly	36 2 51·0	1854, Nov. 10·0, G.M.T.
Perihelion	325 13 54·0 ⎫	Mean Eq. 1854, Jan. 0·0
Node	13 55 38·3 ⎭	
Inclination	2 27 59·1	
Angle of Eccentricity	23 46 42·9	
Log. *a*	0·505894	
Log. *μ*	2·791166	

These elements represent the middle observation with errors,—
(Calculation — Observation) + 0″·2 in Longitude, 0·0 in Latitude.

The foregoing elements were accompanied with an Ephemeris of the planet, extending from Nov. 14 to Dec. 16.

Mr. E. B. Powell writes, that in his paper on the elements of Comet II. 1854 (see *Monthly Notices*, vol. xiv. p. 221), he unfortunately, by an oversight, inserted wrong numbers for the errors of anomaly. The following are the correct numbers and the resulting elements as now furnished by him :—

$v - u = -45''$ for the error of anomaly between the 8th and 13th
$v' - u' = -11''$ —   —   —   8th and 18th
$v'' - u'' = + (1' - 30'')$ —   —   —   8th and 27th

Taking the means of the results afforded by the angles for the two latter intervals, the following elements are obtained :—

<div align="center">

Time of Perihelion Passage	= March 24·03468
Perihelion Distance	= ·2783
Inclination of Orbit	= 81 44 (° ′)
Longitude of Perihelion on Ecliptic...	= 170 16
Longitude of Ascending Node.........	= 316 0
Motion retrograde.	

</div>

At the close of the meeting the President alluded to the curious theory which had been recently started, relative to the

probability of a slow change continually going on in the position of *Saturn's* rings with respect to the body of the planet ; and he pointed out the advantage of examining for this purpose the early observations of the rings, similarly, as on the present occasion, Mr. Hind had done in the paper which had just been read before the Society. He also expressed the interest which he felt in the paper communicated by Mr. Williams, illustrative of some points in Chinese Astronomy, remarking that while such researches were highly creditable to their author, they could not fail, if carefully prosecuted, to be attended with advantage to astronomical science. He then stated that there were three other circumstances to which he would allude in a few words. In the first place he would remark that the results of the Greenwich observations were now published in a separate form, and might be obtained by any person upon applying for them by letter addressed to the Royal Observatory. Secondly, he wished to direct the attention of observers to the fact that, on the Monday following (Nov. 13), the planet *Saturn* would approach very near a small star. He remarked that it was highly desirable to watch such close junctures, inasmuch as the observations of them might occasionally serve to throw light on the physical constitution of the rings of the planet. There was only one recorded instance of a star having been seen in the dark space between the rings and the body of the planet, but the evidence, in support of that observation, could not, perhaps, be regarded as sufficiently worthy of confidence. The President finally adverted, in very brief terms, to the series of pendulum experiments which had been recently executed under his superintendence at Harton Colliery, near South Shields, with the view of obtaining data for ascertaining the mean density of the earth, reserving, for a future occasion, a more detailed account of the operations. He alluded to the attempt of one of his predecessors, Dr. Maskelyne, to accomplish the same object by means of experiments on the deviation of the plumb-line from the vertical direction, occasioned by the attraction of the mountain of Schehallien, in Perthshire. He also noticed the experiments for the same purpose with the torsion balance, adding, however, that he did not consider them to be sufficiently trustworthy, in consequence of the numerous disturbing influences to which operations of so delicate a nature are liable. He then adverted to the experiments with the pendulum, which his friend, Dr. Whewell, and himself had made in the years 1826 and 1828, in the bottom of Dolcoath mine, Cornwall, with the view of obtaining a solution of the same important problem. Notwithstanding several obvious disadvantages under which they laboured, they persevered in the pursuit of their object, until their labours were definitively interrupted by the flooding of the mine. He remarked that in conducting experiments of this nature, there were three essential points to be attended to.—First, there was the comparison of the two pendulums (one of which was to be used at the bottom of the mine and the other at the top) in order to ascertain their physical difference : this was satisfactorily effected by an interchange of the

positions of the two pendulums, any difference in their rate of oscillation being thereby completely eliminated. Secondly, there was the comparison of each pendulum with its clock ; this object was attained by employing the method of coincidences originally suggested by Captain Kater. Thirdly, an accurate comparison of the clock at the bottom of the mine with the clock at the top was indispensable. This was by far the most difficult part of the operations. In the Dolcoath experiment this object was effected by the very inconvenient process of carrying chronometers from the top of the mine to the bottom along a series of ladders. The application of galvanism to astronomical observations had suggested to the President the advantage of comparing the two clocks by effecting a galvanic connexion between them. This is accordingly the method which has been practised in the Harton Colliery experiments : it would be hardly necessary to add that it was, in every respect, superior to any other mode of comparison. The President finally remarked, that until all the necessary calculations were worked out, it was impossible to ascertain the definitive result of the experiments.

Note on the Origin of the Attempts made in the seventeenth Century to derive from physical Principles an invariable Standard of Measure. By the Editor.

The only physical standards of measure, which have been applied to astronomical and other scientific purposes in modern times, are those which have heen furnished by the oscillations of the pendulum and the length of an arc of the meridian. This note is accordingly confined to some remarks on the origin of these two methods.

The pendulum seems to have *first* suggested itself to scientific men as capable of furnishing a permanent standard of measure. Sprat, in his History of the Royal Society (1667), enumerating the inventions and discoveries due to one or other of the Fellows of the Society, mentions the invention of "*a universal standard or measure of magnitudes by the help of a pendulum, never before attempted.*"—(4th Edition, p. 247). In a subsequent part of the same work the author gives a special account of the labours of Dr. (afterwards Sir Christopher) Wren, in the course of which he states that the latter had shown "*that there may be produced a natural standard for measure from the pendulum for vulgar use.*"—(p. 314.)

I am not acquainted with any author who has since alluded to the labours of Wren or any other Fellow of the Royal Society in connexion with this subject. The primary idea of employing the oscillations of the pendulum to obtain a standard of measure is not generally adverted to by scientific writers. Delambre, in his Analysis of the *Horologium Oscillatorium,* pub-

lished in 1673 (*Hist. Art. Mod.* vol. ii.), notices the proposal
of Huyghens to establish an invariable standard of measure
on the basis of the oscillations of the pendulum; but he does not
assert that the suggestion of such a measure was due to the Dutch
philosopher. On the contrary, he refers to a somewhat similar
proposal made by Mouton in his work — *Observationes Diame-
trorum Solis et Lunæ,* Lugd. 1670. It has, however, been
recently asserted, although in somewhat obscure terms, upon the
strength of the passage in the *Horologium Oscillatorium,* above
alluded to, that the invention is really due to Huyghens. This
point will be best ascertained by citing the words of the author
himself. Referring to the advantage of a knowledge of the centre
of oscillation, in finding the length of the pendulum, he thus
remarks:—" Hinc necesse fuit illis, qui, ante hanc centri oscil-
latorii determinationem mensuræ universalis constituendæ rationem
inierunt; *quod, jam inde à prima Horologii nostri inventione,
nobilis illa Societas Regia Anglicana sibi negotium sumpsit* et
recentius doctissimus Astronomus Lugdunensis, Gabriel Mouto-
nus; his, inquam, necesse fuit designare globuli suspensi diame-
trum, vel proportione certa ad fili longitudinem cujus nempe tri-
cessimam vel aliquam partem æquaret."— (*Horologium, Pars
Quarta,* Prop. xxv.) It appears clearly from the foregoing pas-
sage that the idea of deriving an invariable standard of measure
from the oscillations of the pendulum originated with the Royal
Society, having been suggested to some of the Fellows by Huyg-
hens' application of the pendulum to clocks.

Having recently had occasion to examine the MSS. of the
Royal Society, I availed myself of the privilege so liberally ac-
corded to me by that body, to ascertain if any records existed of
the early labours of the Society in connexion with the establish-
mont of an invariable standard of measure. In this research I was
not disappointed. The following extract of a letter from Huyg-
hens to Sir Robert Moray, dated the Hague, Dec. 30, 1661, con-
firms the statement above cited from the *Horologium Oscilla-
torium:* " La fabrique de ma machine [pneumatique] m'a em-
peché quelque temps de travailler aux traités dont vous me de-
mandez de nouvelles. J'ai entre les mains celui de l'horloge,
duquel une grande partie est dediée aux mouvements, *et par-
ticulièrement j'y ai parlé de cet usage du pendule pour la mesure
universelle, dont vous dites qu'on a traité dans votre assemblée."*
(*Letter Book,* vol. i. p. 22.)

Upon referring to the *Journal Book* of the Royal Society, I
found that the question of an invariable standard of measure,
founded on the oscillations of the pendulum, had been discussed at
several of the meetings of the Society, towards the close of the
year 1661 and the beginning of the following year. At the
meeting, held on the 22d of January, 1662, it is stated that the
President, Lord Brouncker, "introduced the history and schemes of
the pendulum experiment, and that a committee, consisting of the
President, Mr. Boyle, Sir William Pettey, Dr. Wilkins, and

Dr. Wren, was appointed to make trials of it." (*Journal Book,* vol. i. p. 46.)

It would appear that the experiments of the Committee were not successful, for, at the meeting held on Feb. 5, 1662, "*Dr. Wren was entreated to think of an easy way for a universal measure, other than the pendulum.*" (*Journal Book,* vol. i. p. 48.)

Viewing the statement of Sprat, in connexion with this passage, it appears extremely probable that Wren was the first who proposed the deriving a standard of measure from the oscillations of the pendulum. The idea was, indeed, worthy of the genius of that distinguished man.

The difficulties experienced by the Committee, doubtless, arose from their ignorance of the centre of oscillation of the different pendulums, as well as from the disturbing effects of the resistance of the air and various other causes.

At length the discovery of a method for determining the centre of oscillation by Huyghens, removed what was considered to be the sole impediment to the success of the pendulum experiment in furnishing an invariable standard of measure. Huyghens announced his discovery in a letter to Sir Robert Moray, dated Nov. 21, 1664. He gives the rule for finding the centre of oscillation of a pendulum, consisting of a homogeneous spherical ball, suspended by a thread or wire. He states that for a pendulum oscillating half-seconds, he found by calculation that the distance between the point of suspension and the centre of oscillation was exactly $9\frac{1}{2}$ inches, Rhinland measure. (*Letter Book of the Royal Society,* vol. i. p. 225.)

The letter of Huyghens, above referred to, was read at the meeting of the Royal Society, on the 23d of November, 1664. In the *Journal Book* it is simply recorded that the letter was read ; but there is evidence to show that the discovery, announced by Huyghens, was justly appreciated by the Society. Oldenburg, in a letter to Boyle, dated Dec. 3d, 1664, states that when the letter of Huyghens was read to the Society, "order was given that it should be punctually registered, to the end that Mr. Huyghens might have his due, and his inventions recorded for his honour to posterity." (Boyle's Works, vol. v. p. 328.)

The Society lost no time in verifying the theorem of Huyghens, respecting the centre of oscillation, by making experiments with pendulums of different lengths, and employing oscillating spheres of different magnitudes. In the first experiments with pendulums, oscillating in half-seconds, the theorem of Huyghens was found to "*be very near the truth.*" In the second series of experiments, when the oscillations of the pendulum were performed in seconds, it was found on one occasion that the string, with the smaller sphere attached to it, was $\frac{1}{10}$ *longer than it ought to have been, according to Huyghens' theorem.* A similar result was obtained in other instances wherein spheres of different magnitudes were employed. This inconsistency between theory and the results of actual experiment was attributed to the resistance of the air.

Thus, notwithstanding the beautiful discovery of Huyghens, respecting the centre of oscillation, it was found that the oscillations of the pendulum could not be confidently employed in furnishing an invariable standard of measure. Hooke, who, probably, felt somewhat piqued, on account of the small share which he had in the inquiry, was the most strenuous objector to the use of this method. He remarked, among other disadvantages which would accompany its practical application, that if, as was in all probability the case, the force of gravity increased in intensity from the equator to the poles, it would necessarily follow that *in proceeding from the equator towards either of the poles, the oscillations of the seconds' pendulum would gradually be quickened.* This suggestion, so characteristic of the genius of Hooke, was announced to the Royal Society nine years previous to the return of Richer from Cayenne.*

Mouton (*Observationes Diametrorum,* &c., 1670) appears to have first proposed the length of an arc of the meridian as the basis of an invariable standard of measure. In the Appendix, p. 427, he suggests a decimal system of measures, the fundamental unit of which he proposes to derive from the value of a minute in Riccioli's length of a degree of the meridian ; and he finds the unit of measure † thus obtained to be equal to the length of a simple pendulum, which makes 1252 oscillations in half-an-hour.

I am indebted to Mr. Sheepshanks for the foregoing statement respecting the proposal of Mouton to derive a standard of measure from the length of an arc of the meridian, not having myself seen the work in which reference is made to that method. ‡

It was soon suspected, as in the case of the pendulum, that the method of deriving a standard measure from the length of an arc of the meridian might be liable to serious objections. At the meeting of the Royal Society, held on the 7th of June, 1682, when the question of measuring an arc of the meridian in England was discussed, the President (Sir Christopher Wren) remarked that, according to his opinion, the best standard of measure " would be a certain part of the length of a degree upon the earth, *if at least upon several accurate trials of the measure of a degree in several latitudes it should be found the same, and not different, as it would be if the body of the earth were oval and not perfectly globular.*" (*Journal Book,* vol. vii. p. 86.)

It appears somewhat strange that it did not occur to any one that the supposed defects of the methods based upon the pendulum and the length of an arc of the meridian might be obviated by confining the experiments and observations to a fixed latitude.

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* Hooke's objections to the use of the pendulum, in furnishing an invariable standard of measure, were stated at the meeting of the Society, held on Dec. 14, 1664.

† Termed by him *the Virga ;* it was equal to the thousandth part of the length of a minute of Riccioli's arc.

‡ Delambre has justly remarked that Mouton's work is very rare.

The planet (31), discovered at Washington, U.S., by Mr. Ferguson, on September 1, has been named by him *Euphrosyne.*

The *Nautical Almanac* for the year 1858 has recently been published. In addition to the usual matter there is a Supplement, containing approximate Ephemerides of the newly-discovered planets, down to *Amphitrite* (29), for the year 1855, and also an ephemeris of De Vico's Comet for the same year. The Supplement also includes numerical values of the elements of the four minor planets, — *Thetis* (17), *Lutetia* (21), *Calliope* (29) and *Amphitrite* (29).

CONTENTS.

A. K. BARCLAY, Esq., V.P., in the Chair.

On the Orbit of 70 *Ophiuchi.* By Eyre B. Powell, Esq.

" Having been for some time engaged in investigating the orbit
of the companion to 70 *Ophiuchi*, I have, at length, arrived at an
ellipse which represents with considerable accuracy the angular
motion of that interesting binary star; and I now do myself the
pleasure of communicating my results to the Society.

" The process by which I obtained a first approximate orbit
was the graphical one invented by Sir J. Herschel: three correc-
tions then gave me the following elements :—

$$\tau = 1806\cdot82$$
$$\varpi = 291^\circ 40'$$
$$\gamma = 46 \quad 2$$
$$\Omega = 294$$
$$e = \cdot566$$
$$n = -3^\circ\cdot676, \text{ and therefore } P = 97\cdot93 \text{ years.}$$

" The errors of position afforded by these elements for 1779,
1804, 1825, 1830, 1833, 1842, and 1852, are all moderate, scarcely
ever exceeding a degree; but the discrepancy between the ob-
served and computed positions for 1802 reaches the enormous
amount of eight degrees. It is worthy of notice that, while the
angle for 1802 is so erroneous, the position for 1804 differs by only
8' from the result of observation.

" The subjoined table contains the variations of the angles of

position, at eight different epochs, corresponding to changes in the preceding elements:—

Table of Variations of Position.

(Variations in minutes of space.)

Epoch.	τ	ϖ	γ	☋	e	n
1779·77	$+\dfrac{33\,a}{4}$	$+\dfrac{71\,b}{60}$	$+21\,k$	$-\dfrac{67\,d}{5}$	$+\dfrac{61f}{2}$	$+\dfrac{25\,e}{6}$
1802·34	$+\dfrac{281\,a}{4}$	$+\dfrac{49\,b}{30}$	$-\dfrac{73\,k}{2}$	$-\dfrac{156\,d}{5}$	$+125f$	$+\dfrac{25\,e}{2}$
1804·42	$+\dfrac{257\,a}{4}$	$+\dfrac{72\,b}{60}$	$-\dfrac{7\,k}{2}$	$-\dfrac{41\,d}{5}$	$+69f$	$+\dfrac{25\,e}{6}$
1825·56	$+14\,a$	$+\dfrac{81\cdot5\,b}{60}$	$-33\,k$	$-\dfrac{79\,d}{5}$	$-53f$	$-25\,e$
1830·5	$+9\cdot5\,a$	$+\dfrac{71\,b}{60}$	$-27\,k$	$-7\,d$	$-37f$	$-\dfrac{25\,e}{2}$
1833·5	$+\dfrac{33\,a}{4}$	$+\dfrac{68\,b}{60}$	$-\dfrac{47\,k}{2}$	$-5\,d$	$-30f$	$-\dfrac{25\,e}{2}$
1842·55	$+\dfrac{23\,a}{4}$	$+\dfrac{62\,b}{60}$	$-\dfrac{27\,k}{2}$	$-\dfrac{3\,d}{5}$	$-15f$	$-\dfrac{25\,e}{3}$
1852·75	$+5\,a$	$+b$	$-3\,k$	$+\dfrac{2\,d}{5}$	$-\dfrac{13f}{4}$	$-\dfrac{25\,e}{2}$

a being tenths of a year; *b*, minutes of space; *k*, degrees; *d*, degrees;
f, hundredths of unity; and *c*, minutes of space.

" The inspection of the variations thus drawn together affords hints with regard to the management of the equations of correction for the different elements. Such equations were formed and combined in a variety of ways, so as to make the values of the corrections depend chiefly on those epochal equations in which the corresponding coefficients were largest. The method of least squares is commonly used to attain the greatest accuracy; but it seemed to me that the foregoing plan would be sufficient to answer the purpose in hand. Thus, then, new and more correct values of the elements were found; but, as a comparison of the angles of position afforded by them, with those of observation, was not so satisfactory as I wished, I again altered some of the elements, and finally adopted the following:—

$$\tau = 1806\cdot92$$
$$\varpi = 291^\circ\,40'$$
$$\gamma = 49\ 56$$
$$\text{☋} = 296\ 30 \text{ and } \lambda = 7^\circ\,29'$$
$$e = \cdot546$$
$$n = -3^\circ\cdot668, \text{ and therefore } P = 98\cdot146 \text{ years.}$$

" The length of the period is, I believe, considerably greater than any hitherto obtained; but an examination of the angular velocities of the star bears out my result.

From 1833   to 1853   ☽ Position = about 19
        1842 — 1852   ——   ─ ─   8¼
     1846·2 — 1852·7   ——   ─ ─   6

" Now, to complete a whole revolution, so as to reach a position angle of 90°, about 23°½ have to be described; and this will require at least 23 years. Hence, as 74 years have already elapsed, the periodic time cannot differ much from 98 years, and is, probably, somewhat greater.

" I annex a comparison of the observed angles of position with those given by the preceding elements.

Table of Comparisons of Observed and Computed Positions.

Date.	Position Observed.	Position Computed.	$P_c - P_o$.	Observer.
	° ′	° ′	° ′	
1779·77	90 0	89 4	— 56	♅
1802·34	334 38	336 10	+1 32	♅
1804·42	318 40	315 27	—3 13	♅
1821·72	157 39	157 19	— 20	Σ
1822·6	153 54	154 25	+ 31	Σ
1825·56	148 12	146 36	—1 36	Σ & S
1828·67	140 18	140 25	+ 7	Σ & H
1830·5	137 28	137 25	— 3	B, H, & D
1832·561	134 21	134 26	+ 5	H & D
1835·56	130 36	130 37	+ 1	Sm
1838·51	126 30	127 19	+ 49	Sm
1842·55	122 24	123 17	+ 53	Sm
1846·017	120 31	120 7	— 24	J
1850·571	116 6	116 12	+ 6	J & F
1852·75	114 3	114 22	+ 19	J
1854·081	113 39	113 16	— 23	J

♅, Sir W. Herschel; Σ, Professor Struve; S, Sir J. South; H, Sir J. Herschel; B, Professor Bessel; D, the Rev. Mr. Dawes; Sm, Admiral Smyth; J, Captain Jacob; F, Mr. Fletcher.

" The epochs pitched upon for the comparison were generally those for which there existed the most trustworthy observations; and in making the selection, I was guided, not only by the circumstances recorded of the measures, but by the angular velocities the latter afforded. Also, in combining the values assigned by different observers, I took the liberty of allotting weights according to the coherence and number of the sets of measures. The position for 1850 is a simple arithmetical mean between the angles of Captain Jacob and Mr. Fletcher, which differ *inter se* by nearly two degrees. I am inclined to think that Captain Jacob's angle for 1854 is rather too high; first, as the angular velocity obtained by combining it with his measure for 1852 is only three-tenths of a degree; and secondly, as I made the position for

1853·586, by 59 fairly accordant observations, equal to 113° 35′, which is but 5′ less than the result of calculation.

"Using the elements and the measured distances for 1781, 1825, 1828, 1830, 1832, 1835, 1838, 1842, and 1850, I computed the corresponding values of *a*, the semi-axis major; the mean came out 4″·48.

"With this value of *a* the following table was formed, showing the comparison of the calculated and observed distances for thirteen epochs. The inclination of the true distance to the apparent one is included in the table, as it may be satisfactory to see its effect in fore-shortening the radius vector. The formulæ used were ,

$$\sin I = \sin \gamma \sin (v \pm \lambda), \quad \rho = a\,(1 - e \cos u), \quad \text{and} \quad r = \rho \cos I.$$

Table of Comparison of Observed and Computed Distances.

Date.	ρ, or Radius Vector.	I, or Inclination. (° ′)	r_c, or Comp. Dist.	r_o, or Observ. Dist.	$r_c - r_o$.	Observer.
1781·74	5·687	31 9	4·867	4·49	+·377	♅
1804·42	2·177	21 6	2·031	2·56	−·529	♅
1825·56	4·873	30 48	4·186	4·00	+·186	Σ
1828·67	5·288	25 44	4·764	4·79	−·026	Σ
1830·5	5·509	23 0	5·071	5·478	−·407	B, Sm, & D
1832·561	5·736	20 6	5·386	5·456	−·070	H & D
1835·56	6·028	16 10	5·79	5·97	−·180	Sm
1838·51	6·273	12 35	6·123	6·351	−·228	B, Sm, & D
1842·55	6·543	7 59	6·48	6·682	−·202	Sm & D
1846·017	6·716	4 18	6·697	6·83	−·133	J
1850·571	6·864	0 30	6·864	6·86	+·004	J
1852·75	6·904	2 31	6·897	6·73	+·167	J
1854·081	6·918	3 50	6·903	6·365	+·538	J

"On glancing at the numbers in the sixth column, the arrangement of the signs of $r_c - r_o$ suggests the existence of error in the elements. It has been asserted that the distance is now decreasing; if this be correct, the node, or the peri-astre, or both, should be somewhat advanced. As, however, the measures of distance are undoubtedly less worthy of confidence than those of position, it appears unnecessary at present to modify the elements with the sole view of reconciling the distances. No weight can be attached to the distance of 1804, as it was obtained by mere estimation in diameters. In 1825 Professor Struve found the projected radius vector 4″·0, Sir J. South 4″·76; the true value in all probability lies between these determinations, and, perhaps, nearer the former than the latter. This opinion is grounded upon two facts; first, that on Professor Struve's testing his distances, he found them on an average about two-tenths of a second too small; and secondly, that the distance in 1828 was only 4″·79, while

from the nature of the orbit the value of r was increasing pretty rapidly. For 1850 Captain Jacob and Mr. Fletcher differ by $0''\cdot366$, and the distance by computation agrees nearly with that obtained by the former. The most offensive discrepancy in the table is that for the current year; but even here it seems probable the orbit is not so far in fault as might at first be fancied. The angular velocity is apparently as small, or even smaller, than it was prior to 1850, and the radius vector lies near both the axis major and the line of nodes; hence there can be but a slight change in the distance.

"Upon the whole, the preceding elements appear to represent the motion of 70 *Ophiuchi* tolerably well; and it is to be feared that, till the companion approaches its peri-astre, but little can be done to fix definitively the most important points connected with the orbit. Probably, however, three or four years' farther observations will put it in our power to improve the position of the ellipse with reference to the line of nodes.

"*Madras, August 9th, 1854.*"

Description of an Eye-piece for Observing the Sun.
By R. Hodgson, Esq.

"The novelty in the arrangement of this eye-piece is, that it permits the full aperture of the object-glass to be used while observing the whole of the sun's disk; an object of importance in solar eclipses and transits of planets across the sun; and it has also the further advantage of allowing the application of all the usual eye-pieces and micrometers.

"It consists in applying to the draw-tube of the telescope a diagonal glass reflector, about two inches in advance of the focal point; the first or reflecting surface being perfectly flat, and the second ground to a concave figure, and left unpolished: a dark glass is, of course, still necessary to protect the eye from the glare of the sun; but a light neutral tint is sufficient.
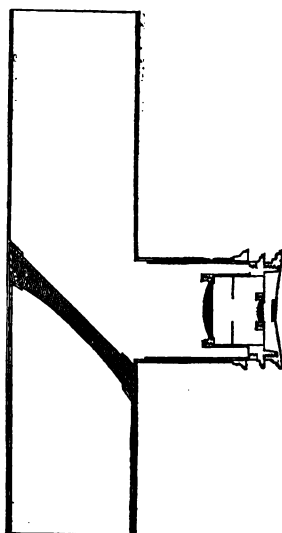
"I have applied it to my 6-inch equatoreal, and also tried it with equal success upon a telescope of 8 inches' aperture.

"*November* 1854."

*Note respecting the recent Pendulum Experiments in the Harton
    Colliery. By G. B. Airy, Esq., Astronomer Royal.*

Since the last meeting of the Society, the computations of the
pendulum experiments in the Harton colliery have been completed,
and the result appears very satisfactory. The pendulums do not
appear to have received the slightest injury or change during the
operations of interchange of position. The final result for the
pendulums is,—

Acceleration at the bottom of the mine is 2·25 vibrations
                        per day ; or,

Increase of gravity at the bottom of the mine is $\dfrac{1}{19190}$
                    part of the whole.

Surveys will now be made for obtaining those data which are
necessary in order to deduce, from this first result, the final result
as to the earth's density. Until these are finished, no approxi-
mate result can be given with confidence : it appears likely,
however, that the resulting density of the earth will be greater
than those hitherto obtained.

December 8, 1854.

On the Satellite of Neptune. By J. R. Hind, Esq.

A discussion of the valuable series of observations of the
satellite of *Neptune,* taken at Valetta in 1852, by Mr. Lassell, has
led me to a result which may not be without interest, as hitherto
only one instance of the kind was known to occur in the
planetary system. There can be no doubt that the motion of the
satellite is *retrograde;* and it offers a far more decided case of
retrograde movement than the Uranian system, the inclination of
the orbit of *Neptune's* attendant being only 29°, whereas the
satellites of *Uranus* revolve in a plane which is inclined to the
ecliptic more than 79°.

From a considerable number of observations I found the place
of the ascending node, in the retrograde orbit, in longitude
175° 40'. It follows, therefore, from the present position of the
planet in the ecliptic, that the apparent orbit of the satellite is
gradually contracting, and will become a right line in 1859, when
the line of nodes is directed towards the earth. Mr. Lassell has
informed me that this is confirmed by his recent observations, in
which the satellite was found to pass nearer to the planet than at
the time of its discovery—a circumstance irreconcilable with
direct movement, since, in that case, the place of the node would
be behind the present position of *Neptune* on the ecliptic, and the
apparent orbit of the satellite would necessarily be opening.

Another inference may be drawn from the observations of

1852, namely, the existence of a very considerable ellipticity in the orbit, though, unfortunately, the determination of its precise amount, as well as a fair approximation to the position of the line of apsides, are scarcely practicable from present data ; and since the apparent ellipse is rapidly approaching to a right line, we must wait 12 or 14 years before the measures necessary for completing the investigation can be obtained.

It will therefore be understood, that the position of the peri-neptunium and the angle of eccentricity in the following elements are very uncertain, though the inclination and node, and also the period of revolution, are, I believe, pretty exact.

<div align="center">

Epoch 1852, November 0·0, Greenwich M.T.

	$\overset{o}{}$
Mean Anomaly	243 32
Peri-neptunium	177 30
Ascending Node	175 40
Inclination	151 0
Angle of Eccentricity	6 5
Period	$5^{d}\cdot 8769$

</div>

If I am right in placing the lines of nodes and apsides nearly in coincidence, it will follow that, at the present date, there will be no very sensible difference between the extreme distances of the satellite eastward and westward of the primary, and this deduction appears to be borne out by a comparison of the measured distances at those points. It will also necessarily result that the satellite must pass nearer to the planet, when it is in the north-preceding quadrant, than it does in the opposite direction—a condition agreeing well with observation. At this epoch the satellite is nearest to the earth when it is to the north of its primary.

I subjoin an extract from a communication with which I was favoured by Mr. Lassell, affording strong evidence in support of the conclusions deduced above :—

" I certainly incline to the opinion that the apparent orbit of *Neptune's* satellite has diminished its conjugate axis since the earlier observations, though all but those of 1852 are made so near the greatest elongation that they could scarcely give even a rude approximation to the true form of the apparent orbit. * * I don't know whether it has occurred to you that the observations at Malta seem to indicate pretty clearly an apparent eccentricity of the orbit,—the satellite passes the planet at its nearest approach on the north-preceding side, something like a second *nearer* than at its nearest approach on the south-following side."

The semi-major axis of the satellite's orbit, as seen at the mean distance of *Neptune*, subtends an angle of $16''\cdot 98$, according to a preliminary discussion of Mr. Lassell's Valetta measures, whence, with the periodic time I have given above, the mass of the planet is found to be $\dfrac{1}{17135}$, and the mean distance of the

satellite from the centre of *Neptune*, 235,800 miles. I believe it will appear, from a comparison of this gentleman's measures generally with those of other observers, that they have a tendency to be in excess rather than in defect of the mean, and consequently the value of the mass, deduced from them, will possibly admit of some diminution: probably $\frac{1}{17500}$ will prove very near the truth.

December 7, 1854.

Note on the Satellites and Mass of Uranus. By J. R. Hind, Esq.

In the *Monthly Notice* of this Society for March 11th, 1853, appears an extensive set of measures of the two brighter satellites of *Uranus*, *Titania* and *Oberon*, taken by Mr. Lassell during his residence at Malta, under circumstances far superior to those which affect the accuracy of such observations in the climate of this country. I have lately worked up those measures with the view of determining the positions of the orbits of the satellites and the mass of their primary. My results are as follow:—

I. *Oberon.*

Radius of Orbit at the Mean Distance of Uranus ...... 45″·20

[or 385,000 miles]

Ascending Node ...... 165° 28′

Inclination 100 34

II. *Titania.*

Radius of Orbit at the Mean Distance of Uranus ...... 33″·88

[or 288,600 miles]

Ascending Node ...... 165° 25′

Inclination 100 34

It will be remarked that these observations give the position of the node and inclination almost precisely the same as they were found by Sir William Herschel more than half a century ago—his numbers being $\Omega = 165° 30′$, $i = 101° 2′$.

I find from the distances of *Oberon*, mass of *Uranus* $= \frac{1}{20642}$, and from those of *Titania* $\frac{1}{20505}$, adopting Mr. Adams' periods of revolution: the mean $\frac{1}{20570}$ agrees very nearly with the value derived from the measures of Sir W. Herschel.

Mr. Lassell's observations indicate with tolerable certainty that the mass of *Neptune* is greater than that of *Uranus* in the proportion of 7 to 6.

1854, *Dec.* 7.

United States Expedition to Chili.

(*Extract of a Letter from Lieut. G. M. Gilliss, U.S.N. to Admiral W. H. Smyth.*)

" Preparatory to the publication of our volume of *Differential Observations on Mars and Venus in Chili*, we are discussing our longitude, and need for this purpose all the corresponding moon-culminations in Europe.

" Congress acted very liberally in the order to publish our results, directing that the whole work should be well printed on good paper, of quarto size, and neatly bound. In all there will be five volumes. Vol. i., containing a full account of Chili (except its political history), and Lieut. MacRae's narrative of the mag-netical expedition across the Andes and Pampas of Buenos Ayres, will fill more than 700 pages. It has 51 maps and plates of land-scapes, natural history, &c. These are in the engraver's hands, and the volume will probably be ready in July.

" Vol. ii., the differential observations on *Mars* and *Venus* wherever made and furnished us. We shall probably fill 500 pages, and the volume will be put to press by May.

" Vols. iii. and iv., meridian observations and zones, each 500 to 600 pages. The instrumental and clock errors have been computed for a large portion of the time; but it is not probable the volumes will be printed in less than $2\frac{1}{2}$ years. The reduction of more than 20,000 *new* stars and their arrangement in the order of right ascension from the zones is a work of much time.

Vol. v., magnetical and meteorological observations, will fill also 500 or 600 pages. This will appear shortly after vol. ii.

" You will perceive that we three in Chili accomplished no small amount of labour; I hope the results may prove acceptable to men of science.

" *Washington, 8th Nov.*, 1854."

On the Resistance of the Ethereal Medium and the Attraction of the Small Planets. By A. J. Angström, Esq., Junior Professor of Astronomy in the University of Uppsala.*

The planetary secular perturbations have more especially en-gaged the attention of astronomers, from the circumstance that the solution of questions, respecting the stability of our solar system,

* This paper is a translation from the Swedish, by A. D. Wackerbarth, Esq. who also communicated it to the Society.

finally depends on the amount of these variations, which, however inconsiderable they may appear, may, in the lapse of ages, accumulate to such a degree, as entirely to change the character of the elements.

Both theory and experience, however, agree that perturbations of such a nature cannot take place within our planetary system. The exception to this rule, which, in the beginning of the present century, was supposed to have been found in the moon's motion, whereof the mean motion had, from the earliest period to which astronomical observations extend, continually increased, disappeared when Laplace succeeded in explaining that phenomenon by a slow change in the eccentricity of the earth's orbit. Hence it appeared that this variation was, like its cause, periodical, and did not even affect directly the mean velocity, but what is called the epoch.

The case is, however, otherwise with comets. Here the largeness of the inclinations and eccentricities by no means excluded perturbations, even of a periodic nature, of such extent as entirely to change the elements of the orbit. A striking example of this is afforded by Lexell's comet, and, probably, also the two comets named after Brorsen and D'Arrest. And such must generally be the case with any comet that comes into the neighbourhood of *Jupiter*. But if the comets can be disturbed in their orbits by the planetary attractions, on the other hand, the masses of the comets are so trifling, that they cannot exercise any sensible influence on the motions of the planets.

Nevertheless, one of the comets, namely that of Encke, has given rise to a supposition which, should it be confirmed, must eventually make itself evident in the motions of the planets, and form a real exception to the received law of the unchangeableness of the mean distances, as subjected only to periodic variations.

It has, in fact, appeared that this comet — independently of the considerable changes which the period undergoes from the planetary perturbations—has, at each revolution, increased its mean velocity in its orbit, so as each time to arrive at perihelion $2\frac{1}{2}$ hours earlier than on the preceding occasion.

This circumstance is easiest explained in the following manner, according to the evidence produced by the learned calculator of that comet, Professor Encke:—there enters, or may be supposed to enter, into the expression for the mean longitude, a term varying as the square of the time ; but then comes the difficulty of accounting for the presence of that term by the general law of gravitation. This has not hitherto been done; and Encke has accordingly had recourse to another means of explanation, namely, the resistance of an ethereal medium.

It is known that the effect which a resisting medium, of the same nature as the atmosphere, would have on a heavenly body's motion, would be to reduce the periodic time and eccentricity of the orbit, whereby there must arise an augmentation in the mean velocity, continually increasing with the time. This was just

what was wanted ; and, on the strength of this experience, Encke assumed, in speciality for his ether, that its density decreases as the square of the distance from the sun ; and, furthermore, that the resistance is proportional to the density of the ether and the square of the comet's velocity.

This supposition is, however, subject to weighty objections. First and principally, if the density of the medium increases up to the surface of the sun, it must hence follow that the ether itself participates in the solar system's progressive motion, as well as in the sun's rotation ; for we have no right to ascribe to the ether the properties of air in one instance, and deny the consequences thereof in another. The sun's rotatory motion is then immediately communicated to the circumjacent condensed ether, and is thence communicated from stratum to stratum, with either the same or a diminished angular velocity. If we assume that the angular velocity diminishes in the same ratio as that of the planets, it follows that the resistance to the planets and to the comets of direct motion is nearly evanescent, but is so much the more increased in the case of comets with retrograde motion. But that this is not the case is sufficiently evidenced by Halley's comet.

The apparent diminution in the volume of the nucleus of Halley's comet, when in the neighbourhood of the sun, has been taken as evidence of the ether's increasing density. But as ether possesses the property of passing freely through all bodies, an increase in its density would not compress, but, on the contrary, if it had any effect at all, would expand bodies. The Newtonian theory for the origin of comets' tails, namely, that they are turned from the sun for the same reason as vapours rise in our own atmosphere, Bessel[*] has shown to be insufficient to account for the various changes in their form.

Since, then, on the one hand, on the hypothesis of the ether's condensation, its resistance, in the case of Encke's comet, must vanish, and, on the other, that hypothesis is insufficient to explain the phenomena as desired, it would seem that it ought to be rejected.

On the density of ether within diaphanous bodies, opinions, as. we know, differ ; since some have considered that density as constant, while others again, like Fresnel, conceive it to be different in different media ; but even if the second of these opinions be the more probable,[†] and thus the molecular forces have the power of altering the ether's density, yet assuredly no such power can be ascribed to gravity.

No traces of the resistance of ether are discoverable in the motions of the planets, and this has been considered as arising from their greater density and less volume ; but this is subject to much

[*] *Astron. Nach.* tome xiii. p. 185.
[†] See the Author's article on the " Plane of Polarisation." *Vetanskeps-Akademicus Förhandlingar*, 1853, No. 6.

doubt. Fresnel's theory of aberration, as well as direct experiments by Fizeau,[*] seem to show that ether, to a certain degree, participates in a diaphanous body's motion, and that in proportion to the said body's refracting power. Thus, the ether's resistance takes place within the body itself, and must, therefore, be independent of the body's volume, but must increase in the ratio of its refracting power. But as this, on the other hand, is compensated by the greater mass which bodies of strong refracting power generally possess, it would seem that we are by no means at liberty to assume any great difference between planets and comets with respect to the ether's resistance.

Meanwhile our knowledge of the component parts of comets is extremely scanty. That they are not either solid or gaseous bodies, is evident from their property of not refracting light. They are, probably, of a powdery consistency. This we may conclude from their property of reflecting the sun-light, combined with the experience of certain observers,[†] that stars, seen through the nucleus itself, appear with even increased brightness. This last-mentioned phenomenon, may perhaps rest on an illusion, as others, for instance Bessel, have observed the contrary; but it may nevertheless be explained by the laws of the interference of light. For if a bright object be occulted by a body of small surface, that object may yet show itself with undiminished lustre behind the occulting body in the direction of the line of junction. Then if the occulting body be itself luminous, its light is added to that of the bright object, which is thus made to appear brighter.

As regards the assumption that the resistance varies as the square of the body's velocity, it is subject to the objection, that the notion of great or small can here only be applied to a velocity with reference to the medium in which the motion takes place; and thus that a velocity, which for air is considerable, cannot be so considered for ether. The speed with which these two separate media transmit undulations, is the best starting-point for a comparison. Thus it seems that the perihelion velocity of Encke's comet is barely one 5000th part of that of light, or about $\frac{1}{3}$ foot per second. This circumstance appears to me of especial weight, when one endeavours to reckon the resistance of ether, which again is still further modified by its probably arising within the body itself.

It would seem to follow, from the preceding considerations, that the density of the ether is throughout space the same, and that its resistance, if it really have any sensible influence, must vary as the first power of the velocity. Professor Hansen, who subjected the different hypotheses on the ether's resistance to a searching examination, has shown that the latter assumption makes but little difference in the numerical value of the resistance, but not so the

* *Comptes Rendus*, xxxiii. p. 349.
† Piazzi, Struve, Realhuber.

former. For if we give the function (called by Encke U*) a
value answering to the observed shortening of the periodic time,
the perihelion distance decreases faster than it would if the ether's
density increased in the neighbourhood of the sun, inasmuch as
the eccentricity in that case diminishes more slowly. Encke has,
indeed, with respect to the appearances of 1822 and 1832, when
the comet was seen only after perihelion, uttered a suspicion, that
the true law of the resistance is as yet undiscovered, and the
deviations at the last appearance seem to confirm this: but
whether the assumption of a constant density would represent the
observations any better, cannot be decided beforehand. Still, to
some extent, this would seem to be the case, since the observations
of 1822 and 1832 tend to give the comet's orbit a greater eccen-
tricity than what results when only observations before perihelion
are used. They would, therefore, better harmonise with that
hypothesis, as to the nature of ether, which makes the eccentricity
decrease more slowly.

The objections to the hypothesis of the ether's resistance,
which have been set forth in the preceding pages, have, however,
awakened in my mind a lively wish to explain, by some other
process, the acceleration which observations prove in the periodic
time of Encke's comet ; so much the more, as the celebrated pro-
poser of that hypothesis himself remarks,—"dass das widerste-
hende Mittel nur als eine Form fur die nothwendig gewordene
Correction anzusehen ist."†

The small planets, whose attractions have not hitherto been
taken into account, have, in this respect, attracted my especial
attention, and I here take the liberty of setting forth the results of
the preliminary investigation that I have instituted on that subject.
Even if it should appear that the observed inequality in the
motion of Encke's comet cannot by this means be explained, yet
every contribution to the knowledge of the mutual relations of
these celestial bodies must possess at least some interest. Encke's
comet will, assuredly, at some future time, throw light on the
extent and masses of the small planets, and thus still further in-
crease the number of truths wherewith it has already enriched
astronomical investigations.

The number known of these bodies, from 4, which was the
amount in the early part of the present century, has, within the
last ten years, risen to 27. The rapidity with which these
discoveries have followed one another, renders it probable that
their real number is very great, although, on account of their
smallness, the greater part of them are likely to remain un-
known to us. Whether one adopt the Olbersian hypothesis,
that all these asteroids are fragments of one large planet, or—be
their origin what it may—that they form a sort of belt or ring

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* [U is used by Encke for the portion of the disturbing force that arises from
the resisting medium.— A. D. W.]

† *Astr. Nachr.*, xii. p. 321.

between *Mars* and *Jupiter*, it is evident that their secular perturbations, which are independent of their places in their orbits, must be the same, whether we consider them as all united in one, or disposed upon several points of a common orbit. The secular perturbations, then, of Encke's comet by the small planets, may give the approximate value of their aggregate mass.

To ascertain the mean position of the 27 asteroids' orbits, I have, from their elements, calculated the mean value of the following quantities :—

$$a\, e \cos . \varpi, \qquad e \sin \varpi, \qquad \sin \Omega \tan I, \qquad \cos \Omega \tan I,$$

and hence obtained,—

For the Asteroids.		For Encke's Comet.	
$\varpi = 65°\ 1'$	$e = 0\text{·}0555$	$\varpi_1 = 157°\ 18\text{·}4'$	$e_1 = 0\text{·}8478$
$\Omega = 138\ 23$	$a = 2\text{·}567$	$\Omega_1 = 334\ 29\text{·}5$	$a_1 = 2\text{·}222$
$I = 4\ 19$		$I_1 = 13\ 20\text{·}7$	

where the elements of Encke's comet are, for comparison's sake, added. Moreover, if we call—

The inclination of the comet's orbit to that of the mean asteroid .... ι
The distance of the perihelion from the ascending node .... ω
The same angle for the mean asteroid's orbit .... ω_1

we have

$$\iota = 17° 32' \qquad \omega = 186° 47'\text{·}5 \qquad \omega_1 = 94° 21'$$

We see, then, that the two orbits form, as it were, two links in a common chain, and that their perihelia are nearly at right angles to one another. This, combined with the large value of ι, shows that the two orbits cannot come into very close quarters with each other. On examining the asteroids' orbits separately, we find that, for some of them whose node-lines fall in the first, and perihelion in the second quadrant, an approach to the comet's orbit is possible. I have, for several of them, reckoned the angles ω and ω_1, and hence the comet's and asteroids' radii-vectores in the line of nodes; but for only three has the difference been less than unity.

	ι	r	r'	Diff.
Irene	18° 48'	2·361	2·811	0·450
Proserpine ...	15 52	3·192	2·662	0·527
Ceres............	18 48	2·023	2·959	0·936

where r is the comet's, and r' the asteroid's radius-vector, in the line of the comet's ascending node. Hence we see that among the hitherto discovered asteroids, there is none that approaches sufficiently near to the comet's orbit to exercise any sensible influence on the comet's motion. It does not, however, hence

follow that there may not be such among the yet undiscovered. Were there one with a mean distance of nearly 2·222, it would produce a periodic inequality of a secular nature, which, in spite of the asteroids' smallness, might be sensible on the integration of the differential formulas.

To return to the secular perturbations which the asteroids cause to Encke's comet. This belongs to the more difficult class of astronomical problems, inasmuch as the projections of the two orbits on the ecliptic intersect each other. Bessel[*] has indeed shown how to find the secular perturbations which a planet causes to a comet, or planet with a comet-like orbit, when the disturbing force can be developed in a converging series in powers of $\frac{r'}{r}$, if r and r' signify the radii-vectores of the comet and planet respectively; and Hansen[†] has discussed the opposite case in which the comet's radius-vector is always less than the planet's ; but these methods both fail when the orbits cross one another.

The following method will not, then, be devoid of interest, although it must be owned that, when the mean distances of the two bodies are nearly equal, it does not lead to very rapidly converging series.

If we designate by m and m' the comet's and disturbing planet's respective masses, and their co-ordinates by

$$x, y, z, r, x', y', z', r',$$

and their distance from each other by ϱ, we obtain, as is well known, the components of the disturbing forces by differentiating the Lagrangian function,

$$R = -m'.\frac{(x.x' + y.y' + z.z')}{r'^3} + \frac{m'}{\varrho};$$

but as the secular perturbations are independent of the planet's co-ordinates, we can reject the first term of this expression, whence we have

$$R = \frac{m'}{\varrho}.$$

Since, moreover, in this expression those quantities only are to be retained which are independent of the planet's mean anomaly $n't$, we may put

$$R_1 = \frac{m'}{2\pi}\int_0^{2\pi}\frac{dn't}{\varrho} \ddagger$$

* *Astron. Nachr.*, xiv., p. 1.

† *Ermittelung der absoluten Störungen in Ellipsen,* 1843.

‡ [If we develope $\frac{m'}{\varrho}$ in a series of multiples of the cosines of the mean heliocentric elongation, and then cast away all the terms involving $n't$, there will evidently remain only the series of constants expressing this definite integral.— A. D. W.]

For the finding of this integral it is necessary to expand $\frac{1}{\varrho}$ in a converging series. For this purpose, if we call the angle between the radii-vectores U, we have

$$\varrho = \surd(r^2 + r'^2 - 2 \cdot r \cdot r' \cdot \cos U),$$

and can put

$$\frac{1}{\varrho} = \frac{1}{(r^2 + r'^2)^{\frac{1}{2}}} + \frac{1}{2} \cdot \frac{(2 \cdot r \cdot r' \cdot \cos U)}{(r'^2 + r^2)^{\frac{3}{2}}} + \frac{1 \cdot 3}{2 \cdot 4} \cdot \frac{(2 \cdot r \cdot r' \cdot \cos U)^2}{(r'^2 + r^2)^{\frac{5}{2}}} + \ldots \ (a)$$

or generally,

$$\frac{1}{\varrho} = \frac{1}{(r'^2 + r^2)^{\frac{1}{2}}} + \Sigma \frac{1 \cdot 3 \cdot 5 \ldots (2 \cdot n - 1)}{2 \cdot 4 \cdot 6 \ldots (2 \cdot n)} \cdot \frac{(2 \cdot r \cdot r' \cdot \cos U)^n}{(r'^2 + r^2)^{n + \frac{1}{2}}}$$

where n takes all positive values from 1 to ∞.

The convergency of this series is determined by the expression $\frac{2 \cdot n - 1}{2 \cdot n} \cdot \frac{2 \cdot r \cdot r' \cdot \cos U}{r'^2 + r^2}$, which must always be less than unity, if r is either greater or less than r', and also even if $r = r'$, unless at the same time $\cos U = 1$. The series (a) may, therefore, be considered as always convergent, except in the case of a collision between the two celestial bodies, in which case, however, the differential equations also would cease to hold.

Since, for perturbations of the first order, if u' be the excentric anomaly

$$d \cdot n' t = \frac{r'}{a'} \cdot d u'$$

we have $\quad \frac{1}{2 \cdot \pi} \cdot \displaystyle\int_0^{2\pi} \frac{d \cdot n' t}{\varrho} = \frac{1}{2 \cdot \pi} \cdot \displaystyle\int_0^{2\pi} \frac{r'}{a' \cdot \varrho} \cdot d u' = \frac{R_1}{m'}.$

It is not necessary for the determination of R_1 to avoid developments proceeding according to the ascending powers of the planet's excentricity e'. If, then, we put

$$\frac{1}{m'} \cdot R_1 = \frac{V^0}{(a'^2 + r^2)^{\frac{1}{2}}} + \frac{V^I}{(a'^2 + r^2)^{\frac{3}{2}}} + \frac{V^{II}}{(a'^2 + r^2)^{\frac{5}{2}}} + \frac{V^{III}}{(a'^2 + r^2)^{\frac{7}{2}}} + \&c. \ldots (b)$$

and reject terms multiplied by e'^3 and the higher powers, we have

$$V^0 = 1$$

$$V^I = -\frac{2}{3} \cdot a' \cdot e' \cdot p - \frac{3}{4} \cdot a'^2 \cdot e'^2$$

$$V^{II} = \frac{3}{4} \cdot a'^2 \cdot r^2 \cdot \{(p^2 + q^2) + e'^2 (4 \cdot p^2 - q^2)\} + \frac{3}{2} \cdot a'^3 \cdot r \cdot e' \cdot p + \frac{3}{4} \cdot a'^4 \cdot e'^2$$

$$V^{III} = \frac{-75}{16} \cdot a'^3 \cdot r^2 \cdot e' \cdot p \cdot (p^2 + q^2) - \frac{15}{32} \cdot a'^4 \cdot r^2 \cdot e'^2 \cdot (25 \cdot p^2 + 2 \cdot q^2)$$

$$V^{IIII} = \frac{105}{64} \cdot a'^4 \cdot r^4 \cdot (p^2 + q^2)^2 + \frac{1}{4} \cdot a'^4 \cdot r^4 \cdot e'^2 \cdot (18 \cdot p^4 + 15 \cdot p^2 \cdot q^2 - 3 \cdot q^4)$$
$$+ \frac{105}{64} \cdot a'^5 \cdot r' \cdot e' \cdot p^3 \cdot (p^2 + q^2)$$

and so on, or if we preserve only the first power of e',

$$
\frac{1}{a'}R_1 = \frac{1}{(a'^2+r^2)^{\frac{1}{2}}} + \frac{1.3}{2.4} \cdot \frac{1}{2} \cdot \frac{a'^2.(p^2+q^2).2^2.r^2}{(a'^2+r^2)^{\frac{5}{2}}} + \frac{1.3.5.7}{2.4.6.8} \cdot \frac{1.3}{2.4} \cdot \frac{a'^4.(p^2+q^2)^2.2^4.r^4}{(a'^2+r^2)^{\frac{9}{2}}}
$$

$$
+ \frac{1.3.5.7.9.11}{2.4.6.8.10.12} \cdot \frac{1.3.5}{2.4.6} \cdot \frac{a'^6.(p^2+q^2)^3.2^6.r^6}{(a'^2+r^2)^{\frac{13}{2}}} + \&c. \quad \dots
$$

$$
- \frac{1}{2} \cdot \frac{3}{2} \cdot \frac{a'.e'.p.2.r}{(a'^2+r^2)^{\frac{5}{2}}} - \frac{1.3.5}{2.4.6} \cdot \frac{3.5}{2.4} \cdot \frac{a'^3.e'.p.(p^2+q^2).2^3.r^3}{(a'^2+r^2)^{\frac{9}{2}}}
$$

$$
- \frac{1.3.5.7.9}{2.4.6.8.10} \cdot \frac{3.5.7}{2.4.6} \cdot \frac{a'^5.e'.p.(p^2+q^2)^2.2^5.r^5}{(a'^2+r^2)^{\frac{13}{2}}} - \&c. \quad \dots
$$

$$
+ \frac{3}{2} \cdot \frac{1}{2} \cdot \frac{a'^3.e'.p.2.r}{(a'^2+r^2)^{\frac{5}{2}}} + \frac{3.5.7}{2.4.6} \cdot \frac{1.3}{2.4} \cdot \frac{a'^5.e'.p.(p^2+q^2).2^3.r^3}{(a'^2+r^2)^{\frac{9}{2}}}
$$

$$
+ \frac{3.5.7.9.11}{2.4.6.8.10} \cdot \frac{1.3.5}{2.4.6} \cdot \frac{a'^5.e'.p.(p^2+q^2)^2.2^5.r^5}{(a'^2+r^2)^{\frac{13}{2}}} + \&c \quad \dots
$$

(c)

where the law of the development is evident.

In the foregoing expression p and q have the following meaning :—

$$r p = \alpha x - \beta y \qquad x = a.(\cos u - e)$$

$$r q = -\gamma x + \delta y \qquad y = a.\sqrt{1 - e^2}.\sin u.$$

$$\alpha = \cos \omega'.\cos \omega + \sin \omega.\sin \omega'.\cos \iota,$$

$$\beta = \cos \omega.\cos \omega' - \cos \omega.\cos \omega'.\cos \iota,$$

$$\gamma = \sin \omega'.\cos \omega - \cos \omega'.\sin \omega.\cos \iota,$$

$$\delta = \sin \omega.\sin \omega' + \cos \omega.\cos \omega'.\cos \iota,$$

where, as before, ι denotes the inclination of the comet's to the planet's orbit, and ω and ω' the distance of the respective orbits' the perihelia from comet's ascending node in the planet's orbit.

In order to obtain the secular variations of the elements we must differentiate R_1 with respect to ω, ω', e, and ι, and then integrate the result so obtained with respect to the time t. Thus, for example,

$$\delta e = -\frac{a.n}{e}.\sqrt{1 - e^2}.\int (A_0 + A_1.\cos u + A_2.\cos 2u + \&c. \dots).dt$$

where

$$A_0 = \frac{1}{2.\pi}.\int_0^{2\pi} \frac{dR_1}{d\omega}.du, \qquad A_2 = \frac{1}{2.\pi}.\int_0^{2\pi} \frac{dR_1}{d\omega}.\cos u.du, \&c.$$

These last integrals are obtained by the application of elliptic functions. For putting

$$r = a.(1 - e.\cos u), \quad \text{and} \quad (\tan \tfrac{1}{2}.u)^2 = \frac{\alpha}{\beta}.(\tan \tfrac{1}{2}.\psi)^2,$$

where $a^2 = \dfrac{a'^2 + a^2 \cdot (1-e)^2}{a'^2 + a^2}$, and $\beta^2 = \dfrac{a'^2 + a^2 \cdot (1+e)^2}{a'^2 + a^2}$,

we get

$$\int_0^{2\pi^*} \frac{d\,u}{(a'^2 + r^2)^{\frac{2n+1}{2}}} = \frac{k}{\sqrt{a}\cdot\beta\cdot(a'^2 + a^2)^{\frac{2n+1}{2}}} \cdot \int_0^{2\pi} \frac{(1 + \mu \cdot \cos\psi)}{\{1 - c^2 \cdot (\sin\psi)^2\}^{\frac{2n+1}{2}}} \cdot d\psi. \quad (d)$$

where $k = \dfrac{a+\beta}{2\,a\,\beta}$, $\mu = \dfrac{\beta-a}{\beta+a}$, $c^2 = \dfrac{(a \div \beta)^2 - 4}{4\,a\,\beta}$.

We have, moreover,—

$$\cos u = \frac{\mu + \cos\psi}{1 + \mu\,\cos\psi} \quad \text{and} \quad \sin u = \frac{\sqrt{1 - \mu^2}\cdot\sin\psi}{1 + \mu\,\cos\psi}$$

and if $\quad 1 + e \cdot \mu = f \quad$ and $\quad e - \mu = g$,

$$1 - e\cos u = \frac{f + g \cdot \cos\psi}{1 + \mu \cdot \cos\psi}; \qquad \cos u - e = \frac{f \cdot \cos\psi - g}{1 + \mu \cdot \cos\psi}$$

The integral, in the right-hand member of equation (d), may, as is known, be expressed by elliptic functions. Furthermore, since the value of $\dfrac{d\,\mathrm{R}_1}{d\,\omega}$, and the other differentials of R_1, are composed of terms, all having the form,—

$$\frac{\mathrm{B}}{(a'^2 + r^2)^{\frac{2\,n+1}{2}}},$$

in which
$$\mathrm{B} = f\,(\cos u,\ \sin u,\ a,\ e)$$

it is easily seen that they may all be expressed by elliptic functions.

To the numerical application of the foregoing, I shall have the honour of returning on a future occasion; and for the present, therefore, confine myself to the following remarks.

In the value of the mean longitude, a term, varying as the square of the time, can arise only from the perturbations of the second order, which belong to the epoch. For if in the perturbations of the epoch of the first order, which vary proportionally to the time, we insert the variations of the elements, which are also proportional to the time, there must necessarily arise a term, varying as the square of the time.

I find, by an approximate calculation, that the variation of the eccentricity of Encke's comet, as far as it is caused by the influence of the asteroids, must, for the present, be negative; so that
$$\delta e = -\,t \times \text{constant.}$$

There arises, mainly through the action of *Jupiter* and *Saturn*, in the value of the epoch, a term proportional to the time, to be

* Mr. Wackerbarth, in a note which we regret we have not room to insert here, has given the analysis of the process by which this transformation is effected.—EDITOR.

added to the comet's mean anomaly $n\,t$. If in this the value of $\delta\varepsilon$ be inserted, there must result a term diminishing with the square of the time. Whether, however, that term be of sufficient amount to account for the observed circumstances in the comet's period, is another question, which mainly depends upon the value we assign to the asteroids' masses and the eccentricity of the assumed common orbit. Against this, it may be urged, that observation does not indicate any very considerable change in the eccentricity of the comet's orbit; and, moreover, that the value of the asteroids' masses cannot unconditionally be assumed greater than what answers to their influence on the nearest planets.* But, on the other hand, it is not necessary to ascribe to that cause alone the whole of the observed diminution of the period, since that can partially be explained by the perturbations of the first order, which the asteroids cause to the period or epoch ; and besides, the elements of the orbit themselves must be somewhat altered if the ether's resistance be ignored. The necessity of a correction for the period of Encke's comet may, indeed, be considered as fully established by observation, but not so the form of that correction. For as far as that proof rests upon the apparitions of 1805, 1795, and 1786, the observations are better satisfied by assuming a periodic correction, embracing about seventy-two revolutions of the comet ; though, it must be owned, that this does not in any way facilitate the explanation of the phenomenon.

In the foregoing investigation, I have assumed that the resisting medium is the same ether, whose existence is presupposed as necessary for explaining the phenomena of light and heat : one might also suppose that the resisting medium is the last stratum of the sun's atmosphere, which, by its reflexion, probably causes the so-called " corona " in a total eclipse. It happens, however, in this case, that that atmosphere must participate in the sun's rotation, and thus its resistance must be insensible.

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* Since the above was written, the Author has had the opportunity of examining, in the *Comptes Rendus*, Nov. 28, 1853, an interesting article, by M. Le Verrier, on the masses of the small planets. Le Verrier considers that, with regard to the relative positions of the common asteroidal orbit, and *Mars'* orbit, the second term of the series, expressing the motion of *Mars'* perihelion, rises to $\frac{1}{4}$ of the first term of the same series. If, then, under this supposition, we take the aggregate mass of all the small planets as equal to the earth's mass, they must, according to M. Le Verrier, cause an inequality in the heliocentric longitude of *Mars'* perihelion, amounting to 11′ in a century. Now, as an inequality of one fourth of that amount could not assuredly have escaped the notice of observers, M. Le Verrier concludes that the sum of the masses of the small planets cannot surpass $\frac{1}{4}$ of the earth's mass. Even if with time this fraction should undergo some considerable modification, still a possibility is thus afforded for determining the limit of the masses of the small planets, which cannot but be of the highest utility in the question of the influence of these masses on Encke's comet.

The following communication relative to the elements of Comet II. 1854, has been received from Mr. Powell. It will be seen that it includes the application of the corrections for aberration and parallax, which were not taken into account in his previous investigations.

Correction of the Elements of Comet II. 1854.
By E. B. Powell, Esq.

Greenwich M.T.	Long. Observed, Corr. for Aberr. and Parallax.	Corr. Applied.	Lat. Observed, Corr. for Aberr. and Parallax.	Corr. Applied.	
April			(South)		
8·06067	49 51 43	+47	5 22 57	+33	The sun's real Long. was used, instead of that in
13·087693	59 0 44	+41	12 33 45	+26	the *Nautical Almanac.*
27·08739	75 31 8	+31	24 29 4	+16	

" The aberration was obtained from the diurnal motion and distance of the comet by means of a table in Vince's Astronomy; and the parallax was found roughly by taking the height and longitude of the nonagesimal from a large celestial globe, and then applying calculation.

" On pushing further the approximations, I found

$$D, \text{ the perihelion distance} \quad = \text{ ·2773579}$$
$$E, \text{ the time of perihelion passage} = \text{ March } 24\text{·}010625 \text{,}$$

which afforded the errors of anomaly,

$$u - v = + 1'', \text{ from 8th to 13th, in an angle of } 10° 41'$$
$$u' - v' = -16'', \text{ from 8th to 27th, in an angle of } 27 11$$

" If D be diminished by ·00003, the errors become

$$u - v = +6'' \quad \text{and} \quad u' - v' = -6''$$

and therefore less on the whole; but I have no doubt of the observation of the 27th being more inaccurate than those of the 8th and 13th; and, consequently, I am inclined to prefer the value of D first mentioned.

" The resulting elements are

$$D = \text{·2773579}$$
$$E = \text{March 24th, ·010625, or 24th, } 0^h 15^m \text{·3}$$

Inclination of Orbit = 82 26 43
Long. ☊   = 315 30 11
Long. ☋ on ecliptic = 167 56 19
          Motion retrograde.

" Working out the comet's place on the 18th,

The error of long. = about 10
The error of lat. = about 4

The very great obliquity of the orbit affects the accuracy of the determination of the inclination, the angle in the numerator of the expression,

$$\cos i = \frac{\sin (\phi - \phi')}{\sin (v' - v) . \sec \lambda . \sec \lambda'}$$

being only $4^\circ\ 19'\ 13''$, using the interval from the 8th to the 27th.

"It may be remarked that Pontécoulant's modification of La Place's method of approximating to the two fundamental elements gives an error of about $-\cdot0056$ in the perihelion distance, and one of about $-\cdot0078$ days, or -11 minutes, in the time of perihelion passage.

"*June 5th*, 1854."

Observations on an important Phenomenon observed with regard to the Hill of Santa Lucia, situated in the City of Santiago de Chile. By Don Carlos Moesta.

" The summit of the hill of Santa Lucia, situated in the capital of the Republic of Chili, is 630 mètres above the level of the sea, and about 60 above the city. Its greatest base is from N. to S., measuring some 5 cuadras (the cuadra is 150 yaras square); its perpendicular line scarcely 2 cuadras.

" The rock of the hill, at first sight, appears to be a basalt; but, on examination, Señor Domeyko has determined it to be a variety of metamorphic porphyry, which he calls ' porfido abijarrado.' The columns of the rock have in various parts not only different directions, but various inclinations. However, the columns in the most southern part of the hill run W. 60° N. with an inclination of 36° to S.E.; the columns at the N. are nearly horizontal, with direction to W. a little to N. The upper part of the hill N.E. and S. is covered with a layer of earth, and not much vegetation; part of it also has broken stones (angular); but to the W., where there is a quarry, the heads of the columns are seen, and it is here where the hill is very steep. It has been necessary to enter into the above details to better understand the phenomenon I am about to refer to.

" In the N. part of the hill was erected, in 1849, the Astronomical Observatory, and it was then believed this was the most favourable position. I here only allude to the house which contains the transit instrument. The axis of the instrument rests on two blocks of stone of $6\frac{1}{2}$ feet, which are placed in solid masonry upon the firm rock, without touching any broken or soft parts of the rock.

" The rock that serves as foundation to this masonry, as I have already stated, is of the before-mentioned columns of porphyry; the direction of these is about in a line with the axis of the instrument, and with but little inclination. In the construction of the instrument, as well as in the fixing it, the greatest care

was taken, so as to compensate for all exterior influences of varia-
tion of temperature, pressure of the air, &c.

"Independently of all this, Mr. Gilliss* has observed from the
period of the erection of the observatory, that the end of the axis
to the E. was constantly elevating itself, and that it was found
necessary to lower this end from time to time, so that the instru-
ment might be maintained in the most convenient position for
observing. This phenomenon has been observed since then until
the present time, and clearly shows that the block on which rests
the extremity of the axis to the east is being raised above the
surface of the earth, and in such a manner, that if we add the
small quantities, the other end has been lowered since the erection
of the observatory in 1849 (these observations of Moesta's were
made in March 1853), the difference will be *a quarter of an inch.*

"So interesting a phenomenon induced me to examine if this
elevation was effected suddenly, that is to say, by movements
caused by earthquakes, which are scarcely sensible on the surface
of the earth, or by a motion slow but gradual.

"I proposed to level the instrument every 12 hours, with all
possible exactitude; and in carrying on these operations, I observed
another phenomenon, not less interesting than the first. I ob-
served that in taking these levels, with the large level of the
transit, the position of the axis was subject to oscillations de-
pendent on temperature.

"Here are the observations I refer to:—

Days. 1853.	Height of Axis to the East.	Height of Axis to West.	Error of Obs.	Prob. Error of Result.	Tempera-ture, Fahr. °	State of Atmosphere.	
Mar. 4	0·007	...	0·190	0·064	70·7	Clear and calm.	
5	...	1·049	0·246	0·058	91·6	—	—
	0·063	...	0·037	0·009	70·0	—	—
8	...	1·794	0·074	0·022	84·0	—	South wind.
	...	1·052	0·206	0·069	62·5	—	Calm.
9	...	2·227	0·067	0·023	89·0	—	—
	...	1·210	0·121	0·040	69·7	—	—
11	...	2·431	0·220	0·065	88·5	Cloudy	—
	...	1·803	0·240	0·064	68·0	Clear	—
12	...	2·369	0·204	0·061	84·5	Cloudy; strong S. wind.	
14	...	2·695	0·050	0·016	68·0	—	Calm.
	...	2·475	0·029	0·009	62·5	—	—
15	...	3·041	0·151	0·045	77·5	Clear	—

"This table contains, independently of the difference of ele-
vation of the two extremities of the axis, the temperature and
state of the atmosphere; and so that we may have an idea of the
exactitude of the observations, I have calculated the medium

* Chief of the United States Astronomical Expedition.

errors of the observations, as well as the probable. Observe that the observations are made at intervals of 12 hours. It is sufficient to look at the table to be convinced that the end of the axis to the E. rises and falls with elevation or depression of temperature; and as, by the construction of the instrument, the temperature influences both ends of the axis in a similar manner, we have to look for the cause of that oscillation away from the instrument; *this cause can be no other than the expansion and contraction of the columns of porphyry,* in which is placed the block that supports the west extremity of the axis; expansion and contraction caused by the heat of the sun during the day and the cold at night.

" Thus, for example, on the 4th and 5th March, the axis rose to the west,

$$1''·056$$

whilst the temperature rose

$$70°·7 \text{ to } 91°·6 \text{ F.}$$

" The same axis fell afterwards

$$1'''·112$$

" Temperature depressed

$$91°·6 \text{ to } 70° \text{ F.}$$

" Little more or less we find this same result in the second group of observations; and if the difference of level in these two groups have not the same relation, with the corresponding differences of temperature, less have we of coincidence in the following group. The 11th and 12th of March the difference of level was only

$$0''·628$$

Temperature depressed from 88°·5 to 61° F.
And it is $0''·596$
Temperature rising $68°$ to $84°·5$ F.

" But this inequality of the differences of level for equal differences of temperature is a proof most truly, that the expansion of the rock produced by the sun's heat is the cause of the rising of the hill.

" The first two groups of observations show that the difference of level was a little more or less than

$$1''·$$

the difference of temperature rising, medium height, to 20°; whilst in the third group the difference of level is only

$$0''·628,$$

notwithstanding the difference of temperature was the same.

" But the last column of the table indicates that at the same time, during the first observations, the sky was clear, and cloudy

during the others. It is evident that, with a clouded sky, the hill or rock cannot become so heated as when it receives the direct rays of the sun; and, consequently, the difference of the temperature of the air and that of the rock must be less in the first case than in the second.

"The temperature of the rock will also depend upon the wind; for example, it will be less when blowing strong from S., or when it is calm. Supposing in both cases the temperature of the air to be the same. Thus, then, there cannot be a constant relation between the differences of level and the difference of temperatures; but it is more than probable that we shall find determined the differences of the temperature of the same rock, in place of the temperature of the air.

"The last group of the observations presents an example in which the level, during 12 hours, remains constant: this fact coincides with the very small change of temperature; in effect, the temperature for the difference being (on the 14th) only $5°\cdot5$; the difference of level was not above

$$0''\cdot22$$

"According to these observrations, there cannot be a doubt that the heat of the sun is the cause of the phenomenon in question; and now it only remains to inquire, how comes it that there is only a partial rising of that part of the hill situated to the west, which elevation is indicated by the level.

"To resolve this question, we must again revert to the topography and geology of the Hill of Santa Lucia.

"I have said that in the W. part of the hill the porphyritic columns are exposed to the immediate influence of the sun, whilst in other parts the hill is covered with a layer of vegetation and broken rock, which protects the said columns from the sun's heat. Moreover, these columns have a N.W. direction, with a strong inclination to the west; so that being exposed from 12 P.M. to the evening to the sun's rays, which fall almost perpendicularly upon the heads of the columns, will necessarily suffer a greater expansion than the great body of the hill situated to the N. and E., which is shaded.

"And thus we must be struck with this wonderful power or force of the sun which causes to rise and fall *periodically* this enormous mass of firm and hard rock analogous to that other force which causes to rise and fall *periodically* the column of mercury in the barometer.

"It will now easily be conceived that in the vicinity of Santiago there could not be a worse place for an observatory than the Hill of Santa Lucia, it being of the greatest importance to have such established at a point where the atmospheric influences have the least possible effect on the instruments."

On the Phenomenon seen during the Total Eclipse of the Sun of November 30, 1853.

(*Extract of a Letter from Capt. Shea.*)

" On perusing the account of the total eclipse of the sun on the 30th November, 1853, at Ocucaja, in Peru (communicated by Admiral W. H. Smyth), in No. 8, June 9th, 1854, of the Royal Astronomical Society's *Monthly Notice*, I find, on referring to my register of the spots passing over the sun's disc, that the spot seen here on the 24th of November, 1853, would occupy the same position in the solar hemisphere as the ' rose-coloured protuberance' described in the account above referred to."

Captain Shea further remarks, that the observations of the total eclipse of the sun, July 28, 1851, appear to him to strengthen the opinion of the identity of the solar spots with the rose-coloured protuberances.

Tables for Facilitating the Determination of the Latitude and Time at Sea by Observations of the Stars. By Charles F. A. Shadwell, Esq., F.R.A.S., Captain Royal Navy. New edition, 8vo. London, 1854.

This new edition of a work well known to the naval profession is distinguished by various improvements, which have a tendency to promote its useful character. Several additional couplets of stars are inserted in Tables I. and II., which, it may be stated by way of explanation to those who have not seen the work, are computed with a view to abbreviate the process for finding the latitude by " Simultaneous Altitudes," or, in other words, by simultaneous observations of the altitudes of two stars.

The author has pointed out the facility with which, under certain favourable circumstances, the same method may be employed for finding the Time, and he has computed a new Table (Table III.) with a view to the promotion of this object. He has also computed a fourth Table adapted to the method " for finding the latitude by the combined altitudes of two stars taken at an interval of time equal to the difference of their right ascensions."

In reference to the last-mentioned method, which was originally given in Lynn's Navigation, but appears to have subsequently fallen into oblivion, the author remarks that, " since the observations required are in themselves very simple, since the practical fulfilment of the requisite condition as to time demanded by the problem is not attended with any particular difficulty, and since the actual computation may be materially facilitated by the use of previously prepared constants depending on the declinations of the stars, there does not seem to be any valid objection to the revival of this problem, and to again submitting it to the notice of the scientific navigator. Moreover, since the period during which the observations may be

made is not restricted to any precise moment, as in the case of meridional altitudes, advantage may frequently be taken of the favourable circumstances of morning and evening twilights, when the distinct definition of the sea-horizon is conducive to the accuracy of the results ; a recommendation analogous to that which attends the method of Simultaneous Altitudes."

Leçons de Cosmographie, Rédigées d'après les Programmes Officiels. Par H. Faye, Membre de l'Institut, &c. &c. Deuxième edition. 8vo. Paris, 1854.

This work contains a lucid exposition of the more elementary principles of astronomy adapted to the present state of the science. The early appearance of a second edition is a sufficient proof of the esteem in which the work is held in France.

Sitzungsberichte der Kaizerlichen Akademie der Wissenschaften. Band XII. (Jänner.) Wien, 1854.

This publication contains three astronomical papers, which it may be occasionally desirable to consult. The first of these is by M. Hornstein : it contains a determination of the orbit of Comet I. 1853, founded upon the totality of the observations of the comet, extending from March 6 to April 11. The second is by M. Littrow, and refers to the points of resemblance which subsist between the orbits of the various bodies composing the solar system. The third, by M. Oeltzen, is devoted to a comparison of the Zone observations of Bessel and Argelander.

Astronomical Observations made at the Radcliffe Observatory in the year 1852. By Manuel J. Johnson, M.A., Radcliffe Observer, Vol. XIII. Oxford, 1854.

The Meridian Observations in this volume, with few exceptions, relate to the Circumpolar Catalogue. The author states that another year will be required to complete the latter on the system laid down of not admitting any star which has not been observed in at least two different years. " This system," he remarks, " though it has prolonged our task, has been found so useful a check against errors and confusions of all kinds, as to afford ample compensation for delay." The author announces that an investigation of the parallaxes of 61 *Cygni* and 1830 *Groombridge,* founded upon observations with the heliometer, will appear in the next volume, which will be shortly published.

Informe sobre las Observaciones hechas durante el Eclipse Solar de 30 Noviembre de 1853, presentado al Senor Ministro de Instruccion Publica. Por Carlos Moesta. Santiago de Chile, 1854.

This account of the total eclipse of the sun of November 30, 1853, does not differ from that communicated to the Society in the course of last summer by Admiral Smyth, and of which a translation was given in the *Monthly Notice* for June 1854 (Vol. XIV., p. 225), except that it is accompanied by a drawing of the appearance of the eclipse which some persons may be desirous of inspecting. It may be remarked, however, that the details of the drawing exhibit a satisfactory agreement with the description given in the text.

Astronomische Waarnemingen gedaan ter bepaling der geografische ligging van Batavia door S. H. De Lange en G. A. De Lange, geografische Ingenieurs von Nederlandsch Indië.

(*Communicated by the Astronomer Royal.*)

This Paper, as the title imports, contains the details of a series of observations instituted with a view to determine the longitude of Batavia. These observations consist chiefly of determinations of the zenith-distance of the moon, and of the right ascensions of the moon and moon-culminating stars. They were commenced in 1851, but were interrupted by the unfavourable state of the weather in the month of October in that year, and soon afterwards by the return of MM. De Lange to Manado. They were, however, resumed in May 1853, and continued to be prosecuted pretty regularly till towards the close of the same year.

The observatory consisted of a hut built of bamboos for the occasion, which could be opened with facility in every direction. The instruments with which the observations were made were a universal instrument by Pistor and Martin of Berlin, and a similar instrument by Repsold.

It was found inconvenient to erect a meridian mark; a bamboo was merely fixed in the direction of W. S. W. at a distance of 200 yards from the place of observation; its azimuthal deviation from the meridian having been determined with the utmost possible accuracy. In determining the error of collimation, the method of making one of the telescopes serve as a collimator to the other was very frequently employed. The adjustment of the telescopes upon each other was very conveniently effected.

In the focus of the telescope two vertical wires were placed very near to each other, which could be very accurately brought upon the wires of the telescope employed as the collimator.

In order that an opinion of the value of the observations may

be formed, a comparison is instituted between the results for right ascension obtained by the two observers on every night of the moon being observed independently by each of them. The following are the outstanding differences:—

—0˙06	+0˙13
—0˙17	+0˙03
+0˙06	—0˙42
—0˙22	+0˙56
+0˙04	—0˙24
—0˙29	+0˙05
—0˙09	—0˙16
+0˙01	—0˙05
—0˙05	+0˙10
+0˙07	—0˙55
	—0˙28

The author estimates the probable error of an observation of right ascension to be 0ˢ˙92, a quantity which, however, he is inclined to think exceeds rather than falls short of the true value.

" The author then proceeds to give an account of the operations of himself and his colleague for determining the longitude from the observed zenith-distances of the moon. The method pursued by them in this instance was suggested by Kaiser, who remarks that it is only applicable in practice when the moon's latitude is small, since the variation in right ascension then takes place almost wholly in altitude.

" In the foci of both instruments," says the author, " I inserted two additional wires on each side of the double horizontal wires which had been already introduced. The instrument being adjusted with all possible precision, the telescope was directed to the moon, when the time of the passage of her limb over each of the horizontal wires was accurately noted and the level of the vertical circle read off. By the aid of a slight previous calculation, it was easy to find the instant of time (and also the corresponding azimuth) when a star in the vicinity of the moon should attain the same altitude. The instrument was then placed in the known azimuth (the vertical circle retaining a fixed position), and the star being brought into the field of view of the telescope by its diurnal motion, the time of its passage over each of the horizontal wires was observed and the level read off. The altitude of the star for the time of observation may be accurately found by calculation, and it is manifest that the apparent zenith-distance of the moon's limb will be equal to the computed zenith-distance of the star, subject to a small correction for the variation of the level corresponding to the interval between the two observations. The following are the advantages of this method:—

" 1°. A small error in the determination of the time does not exercise any sensible influence.

"2°. The effect of refraction is eliminated.

"3°. The method is independent of the unavoidable errors which the observation of zenith-distances entails upon the reading off of the vertical circle. It is also independent of the errors of the instrument, as well as those of the micrometer screws, and of the error of collimation.

"4°. The observation may be repeated several times."

The following process was employed for deducing the longitude from the observations:—

With an assumed longitude ($7^h\ 7^m\ 37^s$ East) the Greenwich mean time of the observation of the moon's limb was found, and the corresponding right ascension and declination of the moon calculated from the *Nautical Almanac.*

From these data the zenith-distance of the moon's centre was finally calculated, which, if the assumed longitude was correct, ought to agree with the observed zenith-distance. If no such agreement was found to subsist, the difference of the two quantities served to indicate the difference of longitude which would result by repeating the calculation with a longitude greater or less by 10^s than the longitude assumed. The author remarks that the agreement of the results obtained in the same night by this method leaves nothing to be desired, and that the probable error of an observed zenith-distance does not amount to more than $1''\cdot3$, which on an average agrees with an error of $2^s\cdot6$ in the longitude deduced from it, a degree of accuracy, he adds, which is not to be obtained by any other method with so small instruments.

The results of the observations are then given. The longitude for each observed zenith-distance is computed, and also the small difference which would result if the assumed longitude were increased by 10^s. The transit-observations next follow with the individual values of the longitude deduced from them. The Paper concludes with the equations of condition for the correction of the longitude based upon a few observed occultations. The method of calculation employed is that given by Professor Challis in the *Nautical Almanac* for 1854.

Telescopic Appearance of the Planet Venus at the time of her Inferior Conjunction, February $28^d\ 1^h\ 45^m$, 1854. By John Drew, Ph. D.

"Having ascertained from the *Nautical Almanac* that on this occasion the heliocentric latitude of *Venus* was nearly at its maximum, I determined to watch her if possible at the time of conjunction; and, as the sky was particularly clear, I succeeded in taking observations of the planet both before and after the occurrence.

"Measurement with the position wire micrometer gave, as nearly as it would admit, $3''$ as the breadth of the illumination: more than half of her circumference was visible, but its extent I

regret that I did not ascertain. On a similar occasion (May 1849) the distance from one cusp to the other was found by M. Mädler, making use of the Dorpat refractor, to be no less than 240° (*Astronomische Nachrichten*, No. 679), whence he deduces 43′ 7″ as the amount of horizontal refraction in *Venus*, or greater than that of our atmosphere by about one-sixth.

"With the object of ascertaining how much of the breadth of the crescent was due to refraction, I calculated the breadth of that portion of her surface which only would be visible, supposing the planet was not surrounded by an atmosphere, and found it to be 0″·67. The difference between this and 3″ is, I apprehend, caused by the refraction of the planet's atmosphere.

"I subjoin the calculation and a drawing of the appearance of the planet at the instant of her inferior conjunction.

		o	′	″
Heliocentric latitude of Venus		3	22	32
Geocentric latitude		8	48	10
Illuminated portion = the sum		12	10	42

"The versed sine of this last quantity is ·0225 : hence ·01125 of her diameter (59″·6) = 0″·67 will be the breadth at the widest part of the illuminated crescent.

"The observations were taken at my observatory, Southampton, with a 5-foot refractor by Dollond.
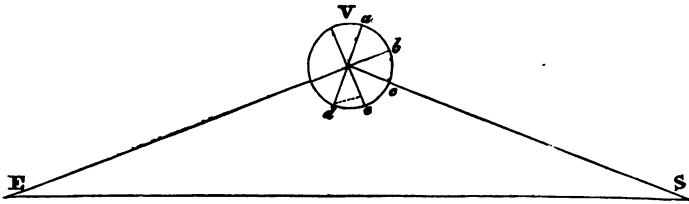
"Let V be *Venus;* E the Earth; S the Sun; ad the boundary of light and darkness; de the illuminated portion, as seen from E $= bc$; but $bc = \angle E + \angle S =$ geocentric lat. + heliocentric lat."

Contribution to the Theory of Elliptic Functions. By Carl John Malmsten, Professor of Mathematics in the University of Uppsala.[*]

In this paper the author gives a new demonstration of the three well-known relations connecting the elliptic function of the third order

$$\Pi (n, k, \varphi) = \int_0^\varphi \frac{d\varphi}{(1 + n \sin^2 \varphi) \, \Delta (k, \varphi)}$$

with those of the first and second order

$$F\,(k,\,\varphi)\,=\,\int_{0}^{\varphi}\frac{d\varphi}{\Delta\,(k,\,\varphi)}$$

$$E\,(k,\varphi)\,=\,\int_{0}^{\varphi}\Delta\,(k,\,\varphi)\,d\varphi$$

The author remarks that Lagrange established the relations between these functions by a process mainly founded on the differentiation of the parameter n; but he shows that the same object may be effected, independently of the differentiation of n, by means of the differential equation of the second order which is satisfied by $F\,(k,\varphi)$ and $E\,(k,\varphi)$.

Elements of Euphrosyne. By M. Winnecke.

Epoch, Sept. 1·0, M. T. Berlin.

$$
\begin{array}{rll}
M & = 299° \ 0' \ 3''\!4 & \\
\varpi & = \ \ 95 \ 13 \ 45''\!1 & \text{Mean Equinox,} \\
\Omega & = \ \ 31 \ 11 \ 59·9 & \quad 1855·0 \\
i & = \ \ 26 \ 53 \ 26·0 & \\
\varphi & = \ \ 13 \ 15 \ 46·2 & \\
\mu & = \ \ \ \ \ \ \ \ 622·091 & \\
\text{Log } a & = \ \ \ \ \ \ \ 0·504102 &
\end{array}
$$

These elements are calculated from the Washington observation of September 2, and the Berlin observations of October 1 and 31.

Elements of Pomona. By M. Bruhns.

Epoch, 1854, Nov. 0·0, M. T. Berlin.

$$
\begin{array}{rll}
M & = 206° \ 32' \ 27''\!6 & \\
\varpi & = 195 \ 46 \ 56·0 & \text{Mean Equinox,} \\
\Omega & = 220 \ 44 \ 20·5 & \quad 1855·0 \\
i & = \ \ \ 5 \ 39 \ \ 2·9 & \\
\varphi & = \ \ \ 5 \ 29 \ 27·6 & \\
\mu & = \ \ \ \ \ \ \ \ 853·694 & \\
\text{Log } a & = \ \ \ \ \ \ \ 0·412470 &
\end{array}
$$

These elements are calculated from the Paris observation of October 28, and the Berlin observations of November 6 and 14.

sent from hence, when the accidental sight of an expression detected an oversight in the reduction of a considerable portion of the comet comparisons,—viz. the measures on the parallel for differences in right ascension by means of the micrometer screw (marked by the letter *d* in column 8) were inadvertently multiplied instead of divided by *sine north polar distance;* and the signs of those of May 20, 21, and June 4, should be changed.

" The present paper represents the observations corrected for the effect of refraction; and I rather hope than expect that it may be received in time to prevent any use being made of the former communication for the elements of the orbit. Moreover, I shall exceedingly regret if the oversight above mentioned should impair the confidence these observations deserve.

<div align="right">Thomas Maclear.</div>

" P.S. Referring to the original register of the observations for the approximate place of star No. 21, where there is a diagram of the field of the telescope, there must have been an error in copying, for the difference in R.A. is there entered + 4ˢ, and in polar distance — 11′ 17″.

" On referring this week to the heavens, I find by micrometric measurement, corrected for refraction, + 4ˢ·455 and — 11′ 23″·18. Applying these numbers to the mean place of star 20 given by Professor Challis, the R.A. of star 21 is 7ʰ 11ᵐ 19ˢ·33, N.P.D. 103° 26′ 24″·55."

Revised Copy of the Observations made on Schweizer's Comet at the Cape of Good Hope. By Thomas Maclear, Esq.

The observations are corrected for refraction only.

The letters *t* and *d* in column 8 denote respectively the right-ascension differences obtained by transits, or by the subtense of the spider lines as given by the micrometer scale.

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 1	7 46 28·10	...	...	+2 50·24	2	1	
	7 56 37·29	—1 46·532	2	...	...	1	*t*
	8 6 17·36	...	...	+3 38·17	2	1	
	8 26 37·32	...	...	+4 25·86	5	1	
	8 37 17·28	—0 34·479	1	...	...	1	*t*
3	7 19 49·87	...	...	+5 12·82	3	2	
	7 42 23·62	—1 12·658	4	...	...	2	*t*
	7 42 23·62	—1 34·739	4	...	...	3	*t*
	7 42 23·62	—1 53·530	4	...	...	4	*t*
	7 56 28·52	...	...	+5 25·34	3	2	
	8 10 29·46	—0 50·447	3	...	...	2	

<div align="center">Clouded on the 2d.</div>

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 3	9 11 36·35	−0 1·623	8	...	...	2	‡
	9 18 32·71	...	...	+ 5 41·45	1	2	
4	6 35 11·59	...	...	− 4 56·02	2	5	
	6 57 18·62	+0 34·348	9	...	...	5	‡
	6 57 18·62	−0 17·970	9	...	...	6	‡
	7 14 42·60	...	...	− 3 19·55	2	6	
	7 22 54·73	−0 3·370	9	...	...	6	‡
	7 35 59·60	...	...	− 3 20·31	2	6	
	8 8 11·72	+0 25·340	5	...	...	6	‡
	8 30 21·54	...	...	− 3 19·85	3	6	
	8 44 49·83	+0 44·486	3	...	...	6	‡
	8 10 53·45	−1 22·002	3	...	...	7	‡
	8 30 21·54	...	...	− 1 4·42	3	7	
	8 50 28·63	−0 58·841	6	...	...	7	‡
	9 13 5·16	...	...	− 1 7·67	2	7	
5	7 0 27·44	...	...	+11 28·42	1	8	
	7 37 17·49	−7 29·912	1	...	...	8	‡
7	7 3 43·40	+1 14·379	4	...	...	9	‡
	7 19 37·18	...	...	−10 13·21	2	9	
	7 36 1·49	+1 22·408	3	...	...	9	‡
	8 3 37·29	...	...	−10 23·67	3	9	
	9 3 57·54	...	...	−10 31·27	2	9	
	9 11 16·84	+1 47·781	1	...	...	9	‡
	7 3 43·40	−0 46·079	4	...	...	10	‡
	7 25 34·21	...	...	− 7 36·70	2	10	
	7 36 1·49	−0 37·838	3	...	...	10	‡
	8 35 1·80	...	...	− 7 52·92	2	10	
	9 11 16·84	−0 12·661	1	...	...	10	‡
8	6 23 36·26	−0 34·350	4	...	...	11	‡
	6 44 36·46	...	...	− 1 28·88	2	11	
	6 23 36·26	−1 1·502	4	...	...	12	‡
	6 50 2·67	...	...	+ 3 59·82	2	12	
	6 23 36·26	−2 20·278	4	...	...	13	‡
	7 0 22·20	...	...	+10 54·81	3	13	
9	7 2 3·47	+2 31·153	2	...	...	13	‡
	7 30 53·78	+2 36·305	1	...	...	13	‡
	7 43 20·78	...	...	+ 6 47·17	3	13	
	8 2 31·62	+2 42·029	5	...	...	13	‡
	8 19 33·00	...	...	+ 6 39·63	2	13	

May 5. Owing to clouds these observations only could be obtained.
    7. Angle of position of comet's tail, 118° 40′. No. 10 is in the comet's tail.
    9. Angle of position of comet's tail, 121° 30′. Nucleus remarkably bright.

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 10	7 11 44·27	...	...	—3 6·40	2	14	
	7 30 39·76	—0 27·602	1	...	...	14	t
11	6 36 25·27	+0 44·368	3	...	...	15	t
	7 0 34·48	...	...	+5 37·06	4	15	
	7 21 27·38	+0 50·129	4	...	...	15	t
	8 12 59·09	...	...	+5 20·65	4	15	
	8 32 5·06	+0 58·440	4	...	...	15	t
	6 36 25·27	—1 37·933	3	...	...	16	t
	7 0 34·48	...	...	+1 2·07	4	16	
	7 21 27·38	—1 32·201	4	...	...	16	t
	8 12 59·09	...	...	+0 45·13	4	16	
	8 32 5·06	—1 23·953	4	...	...	16	t
12	6 41 44·35	...	...	+1 20·44	6	17	
	6 54 57·74	—0 1·515	8	...	...	17	d
	7 2 55·05	—0 0·727	9	...	...	17	d
	7 15 7·60	...	...	+1 14·82	9	17	
	7 36 9·97	+0 2·656	14	...	...	17	d
	8 10 38·13	...	...	+1 5·85	5	17	
	8 19 36·27	+0 7·158	5	...	...	17	d
14	7 27 56·82	...	...	—5 52·53	3	17	
	8 4 32·88	+4 28·769	5	...	...	17	t
	8 42 50·40	...	...	—6 6·18	5	17	
15	6 32 58·31	...	...	+3 34·95	5	18	
	6 49 34·79	—0 37·701	5	...	...	18	t
	7 7 42·58	—0 36·625	5	...	...	18	d
	7 24 52·76	...	...	+3 27·48	5	18	
	7 40 2·46	—0 34·625	5	...	...	18	d
	7 49 22·93	...	...	+3 22·72	5	18	
	8 0 34·68	—0 33·223	5	...	...	18	d
	8 11 40·64	...	..	+3 20·33	5	18	
16	6 20 43·76	...	...	+7 42·41	5	19	
	6 32 35·81	—0 27·532	5	...	...	19	d
	6 40 49·26	...	...	+7 38·85	5	19	
	6 53 34·76	—0 26·166	5	...	...	19	d
	7 3 28·35	...	...	+7 37·75	5	19	
	7 14 24·55	—0 24·868	5	...	...	19	d
	7 21 15·63	...	...	+7 33·79	5	19	

May 10. Clouded after these observations.
11. Angle of position of comet's tail, 115° 40′.   Nucleus less bright; halo round the head more diffused.
12. Angle of position of comet's tail, 117° 30′
14.   ,,   ,,   118 0   Clouds.
15.   ,,   ,,   117 35

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		' "			
May 16	7 37 17·40	−0 23·256	5	...	...	19	d
	7 49 10·05	...	...	+7 28·26	5	19	
	7 57 44·84	−0 22·118	5	...	...	19	d
	8 21 20·18	...	...	+7 23·12	5	19	
17	6 15 6·14	−0 17·635	5	...	...	20	d
	6 16 0·00	−0 22·284	5	...	...	21	d
	6 58 51·77	...	...	−7 56·53	5	20	
	7 5 4·55	...	...	+3 26·32	5	21	
	7 16 43·34	−0 14·416	5	...	...	20	d
	7 18 12·00	−0 19·047	5	...	...	21	d
	7 28 54·65	...	...	−8 0·01	5	20	
	7 34 51·87	...	...	+3 26·49	5	21	
	7 47 36·38	−0 12·801	5	...	...	20	d
	7 48 38·21	−0 17·328	5	...	...	21	d
	8 4 19·63	...	...	−8 0·36	5	20	
	8 9 58·11	...	...	+3 17·34	5	21	
20	6 42 20·46	−0 13·130	5	...	...	22	d
	6 49 23·50	...	...	−0 48·62	5	22	
	6 55 1·77	−0 12·631	5	...	...	22	d
	7 2 55·48	...	...	−0 51·64	5	22	
	7 8 59·68	−0 11·939	5	...	...	22	d
	7 15 43·57	...	...	−0 50·25	5	22	
	7 21 17·66	−0 11·499	5	...	...	22	d
21	6 41 26·83	...	...	+6 3·15	5	23	
	6 49 47·26	+0 15·169	5	...	...	23	d
	6 55 44·28	...	...	+6 3·05	5	23	
	7 2 29·57	+0 15·464	5	...	...	23	d
	7 9 28·82	...	...	+6 1·23	5	23	
23	6 35 26·93	...	...	+7 51·42	5	24	
	6 48 36·57	−0 2·862	5	...	...	24	d
	7 0 21·44	−0 2·726	5	...	...	24	d
	7 9 41·51	−0 2·275	5	...	...	24	d
	7 18 44·82	...	...	+7 41·29	5	24	
28	6 27 55·21	...	...	+4 55·45	5	25	
	6 45 59·48	−0 31·904	5	...	...	25	d
	6 56 48·34	−0 31·935	5	...	...	25	d
	7 6 1·61	...	...	+4 51·99	5	25	
29	6 13 20·56	−0 6·919	5	...	...	25	d

May 17. Angle of position of comet's tail 118° 45'.
  20. The comet is faint, partly owing to the moonlight. The tail barely distinguishable.
  23. Thin clouds. The comet is very indistinct.
  29. The comet is very faint.

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 29	6 20 29·69	...	...	+2 32·25	5	25	
	6 29 17·02	−0 6·573	5	...	...	25	d
	6 35 10·81	...	...	+2 30·35	5	25	
	6 40 52·64	−0 6·273	5	...	...	25	d
31	6 24 32·60	+0 38·938	5	...	...	25	d
	6 31 0·12	...	...	−2 5·16	5	25	
	6 37 46·37	+0 38·917	5	...	...	25	d
	6 46 57·97	...	...	−2 9·65	5	25	
	6 54 6·78	+0 38·865	5	...	...	25	d
June 1	6 27 12·98	...	...	+10 1·96	5	26	
	6 34 57·26	−0 50·387	5	...	...	26	d
	6 41 52·68	...	...	+9 58·95	5	26	
	6 50 17·87	−0 49·796	5	...	...	26	d
	7 14 34·44	−0 48·083	3	...	...	26	t
	7 14 34·44	+1 0·237	3	...	...	25	t
	7 24 49·65	...	...	−4 23·62	5	25	
2	6 27 25·54	+1 18·965	3	...	...	25	t
	6 45 16·64	...	...	−6 26·94	4	25	
	7 1 22·02	+1 19·253	2	...	...	25	t
	6 27 25·54	−1 24·261	3	...	...	27	t
	6 45 16·64	...	...	−2 53·07	4	27	
	7 1 22·02	−1 23·896	2	...	...	27	t
3	6 36 53·39	...	...	+5 54·40	5	26	
	6 45 8·39	−0 11·989	5	...	...	26	d
	6 52 20·58	...	...	+5 53·80	5	26	
4	7 7 22·67	...	...	+3 53·75	1	26	
	7 13 12·69	+0 5·810	2	...	...	26	d
	7 25 19·64	...	...	+3 47·42	2	26	
5	6 34 48·27	...	...	+2 4·91	3	26	
	6 45 21·34	+0 22·608	5	...	...	26	d
	6 55 12·79	...	...	+2 4·20	3	26	
7	6 35 58·61	...	...	−1 22·38	3	26	
	6 47 3·93	+0 54·498	10	...	...	26	d
	6 58 43·86	...	...	−1 24·30	5	26	
	7 16 36·13	+0 54·550	10	...	...	26	t
	7 31 40·34	...	...	−1 26·34	2	26	
8	6 49 9·52	+1 8·199	5	...	...	26	t
	7 7 54·37	...	...	−2 59·49	10	26	
	7 26 44·26	+1 9·505	3	...	...	26	t

June 1. The measurements diminish in precision owing to the faintness of the comet.

    3. The comet is very faint.

    4. Generally clouded. These observations only could be obtained.

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
June 9	6 23 37·43	...	...	−4 22·13	5	26	
	6 38 11·42	+1 23·619	7	...	...	26	
	6 56 13·48	...	...	−4 23·55	5	26	
10	6 23 48·76	...	...	−5 47·12	5	26	
	6 39 39·45	+1 38·681	5	...	...	26	*t*
	6 54 45·86	...	...	−5 48·28	5	26	
11	6 34 14·35	...	...	+4 51·73	5	28	
	6 45 47·37	−0 23·449	5	...	...	28	*d*
	6 55 0·64	...	...	+4 50·24	5	28	

June 10. The moonlight is now diminishing the visibility of the comet.

" *Royal Observatory, Cape of Good Hope,*
Nov. 12th, 1854."

On the Telescopic Appearances of Saturn with a 7½-inch Object-Glass. By the Rev. W. R. Dawes.

In the spring of last year (1854) I availed myself of an opportunity of increasing the optical means in my possession, by the purchase of a 7½-inch object-glass, having a focal length of nearly 9½ feet. It is the work of Mr. Alvan Clark, of Boston, U.S., who has long been known in that city as a most successful painter of portraits, but took to the manufacture of telescopes as an amateur. Being dissatisfied with reflectors, on which he commenced his operations, he attempted the manufacture of object-glasses; and succeeded so well, that in the autumn of 1851 he communicated to me the places of some new and very close double stars, which he had discovered with glasses whose apertures were 4¼ and 5¼ inches. In the following year he completed an object-glass of 7⅛ inches aperture for the observatory at Williams's College, which was tried at the Harvard Observatory by the Messrs. Bond, and highly approved: immediately after which he commenced one of 7½ inches aperture, intended to be retained and mounted equatoreally for his own use. At his request I sent him some extremely difficult tests, selected from Mr. Otto Struve's *Pulkova Catalogue;* several of which have a central distance of little more than half a second, and some even less. Yet of all these I soon received from the ingenious maker (who has also proved himself an acute observer) perfectly correct diagrams; together with the places of one or two extremely difficult new double stars which he had discovered with this glass. As a-specimen of these, I may mention 95 *Ceti,* which is at present favourably situated for observation. Though unwilling to part with this glass, Mr. Clark consented to let me have it to try against my Munich telescope; and in March 1854 it arrived, with its tube, finder, and eye-pieces.

Though the crown-glass has a considerable number of small bubbles, the performance of the telescope is not sensibly affected by that circumstance. In other respects the materials are good; and the figure is so excellent, and so uniform throughout the whole of the area, that its power is quite equal to anything which can be expected of the aperture; and, consequently, both in its illuminating and separating power, it is decidedly superior to my old favourite of 6¼ inches aperture. As a specimen of its light, I may mention the companion of *υ Ursæ Majoris* as having been pretty steadily seen with it; and also that I have never seen *Saturn* under tolerable circumstances during the present apparition without detecting *Enceladus*, even when at or very near his conjunctions with the planet. When exterior to a tangent to the extremity of the ring, this satellite has frequently been perceived as soon as my eye was applied to the telescope. Last spring it was seen several times in strong twilight; for instance, on March 16th, 17th, and 20th, at about 7ʰ G. M. T. In separating power, the glass is competent to divide a sixth-magnitude star composed of two equal stars, whose central distance is 0″·6.

I have thought it proper to premise thus much respecting the performance of the telescope, that a correct idea may be formed as to the degree of dependence to be placed upon the views it has afforded me of *Saturn;* the special subject of my present communication, to which I will now proceed.

1. *The outer Ring,* A. The interior edge of this ring is decidedly its brightest part: its light rapidly fades away towards the middle, where there is a very dark, narrow, well-defined line concentric with the ring, and about one-fifth of its breadth by careful estimation. This line has been always seen when the air was in a tolerably good state, and much more readily than last year. On the 26th November, 1854, it was traced more than half way round towards the ball, and was equally well seen at both ansæ. I have recorded on 10th January, 1855, "I am surprised at the positiveness of the dark line near the middle of this ring. It was well seen with every power from 355 to 1000." This is now the fourth apparition of *Saturn* in which I have noticed this dark line, and it does not appear to me to have varied in its position on the ring, or in its breadth and depth of shade.

2. *The interior bright Ring,* B. The concentric shaded bands on this ring have been on two or three of the most favourable occasions very well brought out. On this appearance I find the following notes in my journal:—

"1854, Nov. 26. The ring B is decidedly in *stripes,* and they are not *regularly* darker from the exterior one inwards. About one-fifth of the breadth of the ring, from its exterior edge, is very bright; then a narrow stripe is lightly shaded; immediately within that is a stripe decidedly lighter, though not so bright as the exterior fifth; next to that is a considerably darker stripe, and then a *much* darker one extending nearly to the interior edge,

where there is a *very* narrow *bright* line, far less decided than it was in 1851 and 1852."

" Dec. 7. By brief views the step-like character of the shading on ring B is visible; and I think the *outer* shaded band is *darker than the next interior one*, as I noticed one night before."

" 1855, Jan. 10. The bands of shading towards the interior edge of this ring are occasionally well brought out; and I think the *second* from the outside is not quite so dark as the first,— at least in some parts of it, for I doubt if it be quite uniform. The narrow bright line at the interior edge is visible, but is not, I think, so bright as it was the two previous apparitions."

3. *The obscure semi-transparent Ring*, C, has been very well seen on several occasions; and I have noticed nothing remarkable about it except the occasional variations of its tint in different parts. Respecting this I have recorded as follows:—

" 1854, Sept. 26. The dark ring is plainly seen, and appears to-night of the same tint at both ansæ. Its semi-transparency is very obvious across the ball, the edges of which can at times be distinctly traced down to the inner edge of ring B."

" Dec. 26. The dark ring is remarkably clear: the following end is *ruddier* than the preceding."

" 1855, Jan. 10. The ring C is wonderfully well seen in general: rather ruddy on the preceding side, slate-coloured on the following side. The ball is seen plainly, though faintly, through it."

4. *The Ball.* Of its appearances I have the following notes:—

" 1854, Sept. 26. The belts on the ball are not very distinct. The southern boundary of the broad dark belt, which is immediately south of the equator, is *not uniform*, or parallel to its northern edge. The belt, therefore, varies in breadth in different parts, and is at present ($13^h 45^m$ G. M. T.) broadest near the eastern edge of the ball. There is a very narrow light line seen interruptedly crossing the belt from east to west, a little south of its middle. The rest of the southern hemisphere is nearly uniform in colour, except that round the south pole is a belt of rather darker tint, and at about $40°$ of south latitude there is a very narrow belt less dark than the polar one."

" Dec. 7. The annexed sketch " (in the journal) " shows the form of the shadow of the ball on the ring B. It does not extend to the ring A at all; but I think a very small portion ($0''·2 \pm$) of the southern edge of the ball is projected upon A."

" Dec. 16, $12^h 30^m \pm$ G. M. T. The south pole, or rather the most southerly part of the ball, is very dark,—much darker than the ring A, and I think rather darker than the broad belt near the equator. This renders the contrast with the small visible portions of its shadow less evident. I feel pretty sure that the southern edge of the ball encroaches a trifle on the ring A. There are no distinct and well-defined belts on the ball now."

" 1855, Jan. 10, $9^h \pm$ G. M. T. The whole of the southern hemisphere of the ball is ruddy, and the parts near the equator

and at the southern edge are the darkest. Examined very care-
fully, and with all the various powers" (extending from 355 to
1000), " the position of the southern edge with respect to the
edges of the rings at that part. The edge of the planet is so dark
that it gives the impression sometimes of having a dark line
there marking its contour. This darkness of the shading, at the
very edge of the ball, renders it difficult to distinguish it from the
division between the rings. But after long and careful exami-
nation, I am satisfied that *the ball extends over the division, and
encroaches* o"·2 *or* o"·3 *on the ring* A. By carrying my eye
across from the black division on one side to the other, I can see
that, if continued in an uninterrupted line, it would *cut off a thin
slice from the edge of the ball.* With very high powers (705 to
1000) the difference of colour of the southern edge of the ball, and
the ring A at that point is more marked than with the lower
powers; and long scrutiny with them confirms my impression that
the ball encroaches slightly on A."

" 10^h 36^m ± G. M. T. Applied an excellent Huygenian eye-
piece, giving power 860. It is admirable. The difference of
colour of the southern edge of the ball and the ring A is obvious;
and there is no doubt at all of the slight encroachment of the ball
on its interior edge. Finding the light of the planet produce a
very unfavourable effect upon my eye while endeavouring to
estimate the degree of encroachment of the ball on A, it occurred
to me to apply my solar eye-piece for the purpose of excluding the
rest of the ball and rings, and leaving visible only the southern
portion of the ball and the adjacent portion of the rings A and B.
Power 506 (the highest, a double-convex lens). The effect is
admirable. My eye having rested upon it for some time, the
outline of the southern edge of the ball was far more distinctly
seen than before, and leaves no doubt of its encroaching on the
interior edge of A, to about o"·3 by careful estimation. At times
a little mottling can be discerned very near the southern limb of
the ball. Its colour is very different from that of ring A; and it
completely interrupts the black division which comes sharply up
to the ball on both sides of it."

5. *The Shadow of the Ball on Ring* B. On this appearance
I have noted as follows:—

" 1854, Sept. 26. The shadow of the ball on ring B is nearly
a straight line."

On Sept. 29 the projecting portion of the shadow, which has
been noticed the last two or three years, was seen for the first
time this season on the eastern side of the ball; cutting off the
acute point of the ring B in-
tercepted between the edge
of the ball and the black di-
vision, as at *a* in the sketch,
in which the appearance is
much exaggerated.

At the same date I have remarked,—" I doubt if the shadow

of the ball on ring B is really a straight line, though nearly so.
It seems to be a little curved
towards the southern end of it,
close to the division." In the
place indicated the edge is *con-*
vex towards the ball.

"Nov. 26. Only·a *very* narrow line of shadow from the ball
falls on the west side, but there is a curious angular projection in the
shadow on both the west and east sides of the apex of the ball."

"Dec. 7. The annexed sketch shows the form of the shadow

of the ball upon ring B." (Exaggerated in the sketch as respects
the *size* of the shadow.)

6. *The Satellites.* I have usually estimated *Tethys* to be
brighter than *Dione*, even when it has been nearer to the planet.
This was remarkably the case on the
10th of this month, at $7^h \pm$ G. M. T.
when both the satellites were near
their greatest western elongation.
At $11^h 19^m$ G. M. T., *Dione, Tethys,*
and *Enceladus*, formed an equila-
teral triangle south-preceding the
western end of the ring, thus,—
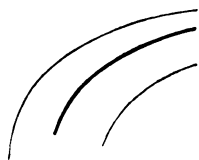
In No. 929 of the *Astron. Nachrichten,* is a most interest-
ing account by Professor Secchi of the appearance of *Saturn* in
the Munich equatoreal refractor, recently erected at the Obser-
vatory at Rome. The dimensions of the telescope are the same as
those of the Dorpat refractor, the aperture of the object-glass
being 9 Paris inches. The Professor characterises the night of
Nov. 19 as one of extraordinary excellence, and doing full justice
to the telescope. He describes the dark line on ring A as being
just like a *pencil line* drawn upon it, which perfectly agrees with
the views I have had of it since 1851; and with my description as
"narrow, very dark, but not black." And when the dusky hue
of the ring A is considered, it seems probable that this line would
appear almost black if contrasted with a much brighter ground,
such, for instance, as the exterior edge of the ring B. It deserves
to be remarked that a dark line, precisely similar to this in appear-
ance and situation, was seen on the *northern* surface of this ring,
in the year 1838, by Professor Encke; and by Mr. Lassell and
myself in 1842 (when we were not aware of Encke's observation).
It may not be a *division* in the ring, as it was then supposed to be;
but, if it is not, it is certainly extraordinary that precisely the
same appearance should exist *on both surfaces of the ring,* and

should be, as it would seem, a permanent phenomenon in respect of its situation on the ring, and the darkness of its shade.

Professor Secchi has also described the step-like concentric bands of shading on ring B, exactly as they were described by myself on October 26, 1851; and as I have occasionally seen them almost precisely in the same way to the present time, it may fairly be concluded that they form a permanent feature of this ring. The Professor does not notice the *comparatively bright line* at the interior edge of B, which seems to me to render that edge pretty definite, though it is certainly less bright now than it was two or three years ago.

In one important point the impression received by Professor Secchi differs decidedly from my own, as stated in the present paper: viz. the place to which the southern edge of the ball is seen to extend on the rings. He states that the opening of the ring is such, that *the upper edge of the ball exactly touches the interior edge of the black division between A and B*, which was visible throughout the whole of its elliptic perimeter. It is singular that, on the 26th of September I arrived at precisely the same conclusion; but the state of the air was not such as to permit the advantageous use of high powers; and my subsequent observations, under much better circumstances, and especially on the 10th of this month, convinced me that my first impression was erroneous, or that a change to a considerable, and in fact *unaccountable*, amount had taken place.

The *first* satellite of *Saturn* (now usually called *Mimas*) is stated by Professor Secchi to have been seen on November 19th, near its greatest western elongation; having been found by putting the planet nearly out of the field, and afterwards seen steadily with the planet in full view. It is surprising that he does not mention *Enceladus*, which must have been close to *Mimas* at that time, if the latter occupied the place indicated. As my telescope has not shown me *Mimas*, I cannot say where that satellite might have been; but my own observations prove that *Enceladus* occupied precisely the situation which the Professor has ascribed to *Mimas;* and I cannot but think it probable that further observations may have convinced him that it was not the *first*, but the *second*, satellite which he saw.

The *bright zone on the ball*, which commences almost precisely at the equator, and extends northwards as far as the ring permits it to be seen, forms one of the most conspicuous features of the planet. It has been repeatedly referred to by Professor Secchi, as *caused by the reflection of the sun's light from the surface of the ring.* Two considerations seem to me to be quite conclusive against its arising at all from that cause. One is, that this bright zone occupied precisely the same situation, and was very conspicuous, *when the plane of the ring passed through the sun.* (See " Remarks on the Planet Saturn," by the Astronomer Royal, in the *Greenwich Observations* for 1848, p. 44.) The other is, that the reflection of the sun's light from the *southern* surface of

the ring, which now receives it, must necessarily fall upon the *southern* hemisphere of the ball, which has been remarkably dark ever since the southern surface of the ring has been illuminated; while the bright zone lies wholly in the *northern* hemisphere.—The remarkable obscurity of the southern hemisphere at the present time seems to indicate that the effect of the reflection from the surface of the ring is quite inappreciable as seen from the earth.

Wateringbury, Jan. 11, 1855,

Postscript.

" Jan. 14. The night proving fine, I again carefully examined *Saturn*, and made the following entry in my journal:—

" ' 12ʰ 45ᵐ G. M. T. *Saturn* is very fine at times, though about $3\frac{3}{4}^h$ past the meridian. It bears 705 very well; and with this power I have no doubt of the southern edge of the ball extending over the division between A and B, and encroaching a trifle on the interior edge of A. With low powers (355 or less) there is sometimes an appearance of the division extending across; but I am persuaded that this arises from the combined effect of · the division coming up on each side so near the apex, and the *very* deep tint of the apex itself, which I think is *darker* than the darkest part of the broad belt close to the equator of the planet. It is certainly much darker than the ring A.' "

On Rating Chronometers by Lunars. By H. Toynbee, Esq., Commander of the Gloriana, East Indiaman.

Previous communications from Captain Toynbee on this subject will be found in the *Monthly Notices*, vol. ix. No. 7, and vol. xiv. pp. 19 and 243. In the present paper he shows that lunar distances taken from the stars and planets may be still more advantageously employed for determining chronometer errors and rates than those taken from the sun.

His method is to take several sets of good lunars on each side of the moon, if it may be, on consecutive days, and from the *mean* of the whole to deduce a chronometer *error* corresponding to the *mean epoch*. This is to be done every month. The *rate* is determined by a comparison of the *error* of each month with the error of the preceding month, and the examples adduced by Captain Toynbee of his own practice, and of that of Captain Quihampton of the Tudor, are exceedingly satisfactory.

It is to be feared that, partly from the practical difficulty attending lunar observations, and partly from the extended use of chronometers in navigation, this excellent and independent method of finding the longitude is going out of use. Yet in long voyages it must always be dangerous to rely solely on chronometers, unless they are numerous, and have been rated *with reference*

to the temperature, which is not the practice of private raters. Even then, there are so many causes which may operate to change the rates of chronometers at sea, that a conscientious officer ought never to risk his ship and the lives of his crew upon the going of his chronometers, without the additional check and security which he derives from lunars.

In his last voyages, Captain Toynbee has relied chiefly on lunar distances taken from the stars and planets, and they are generally taken on each side of the moon the same night. When distances are taken from both sides of the moon, most of the instrumental errors affect the results contrary ways, and almost to the same amount. Thus the index error and want of parallelism in the dark glasses disappear from the mean of east and west distances; and if the arc is nearly the same in both cases, even the defective plane of the index-glass, the want of perpendicularity in the index and horizon-glasses, and the error of the arc of the sextant, are nearly eliminated, or at any rate greatly attenuated. The sextant is so difficult an instrument to make and to adjust correctly, that only the very best artists can be trusted; and, after all, the observer should use it in such a way as to reduce as much as possible the effect of unavoidable and unknown errors : for this purpose he can scarcely follow a better example than Captain Toynbee.

The errors of the lunar tables are sometimes larger than would seem likely from Captain Toynbee's instances, yet his method of extending the observations (from which his mean distance is included) over so large an arc will tend to diminish even this effect, which cannot be completely guarded against. The *number* of observations gets pretty well rid of *casual* error, either of observation or of computation; and the instrumental errors are compensated by making them tell contrary ways.

The following instances will suffice to show the system pursued by Captain Toynbee and its results.

By a mean of 10 distances on each side of the moon on March 6, 7, 8, 9, 10, 11, chron. Dent, 1759, was found,—

<div style="text-align:center">

1854, March 9, 14^m $39^s\!\cdot\!6$ fast on G.M.T.

</div>

By 18 distances taken similarly between May 1 and May 10, the chron. was found,—

<div style="text-align:center">

1854, May 6, 22^m $38^s\!\cdot\!2$ fast on G.M.T.

</div>

By 14 similar distances taken between August 29 and Sept. 6, the chron. was found,—

<div style="text-align:center">

1854, Sept. 2, 40^m $35^s\!\cdot\!6$ fast on G.M.T.

</div>

By 8 similar distances taken between Sept. 27 and Oct. 6, the chron. was found,—

<div style="text-align:center">

1854, Oct. 1, 44^m $25^s\!\cdot\!9$ fast on G.M.T.

</div>

By 12 similar distances between Oct. 31 and Nov. 3, the chron. was found,—

1854, Nov. 1,   48^m 9^s·9 fast on G.M.T.

By 8 similar distances between Nov. 30 and Dec. 1, the chron. was found,—

1854, Dec. 1,   52^m 19^s·8 fast on G.M.T.

The partial results are good, but not extraordinary. The excellence of the *means* arises from the number of observations and the compensation of the opposite errors.

The chronometer seems to have performed with great regularity, as the rates from the above data are,—8^s·4 ; 9^s·1 ; 7^s·9; 7^s·2; 8^s·1 : all gaining.

The computed error of the chronometer from the above *errors* and *rates*, deduced solely from lunar distances, was verified at Calcutta and the Cape by the time-ball, and on the return to England by the maker; the differences amounted only to a few seconds.

The observations from which these results are deduced were taken while the moon was between new and full, and generally between 6^h and 10^m P.M.,—a time which best suited Captain Toynbee's convenience. He proposes hereafter to try morning observations, by which an independent error and new rate will be determined every fortnight. Whether this will repay the trouble depends on the chronometers, which, if good, and more than one in number, should keep the Greenwich M.T. for a month pretty safely. Towards the end of a voyage, the error of the chronometer cannot be too carefully determined, and once a fortnight is not too often.

Captain Quihampton says that he should have been a degree out of his reckoning if he had relied on his shore-errors and rates; but by his lunar rates he made the Island of Flores and the Islands of Scilly within a very few miles.

On the Orbit of α Centauri. By Eyre B. Powell, Esq.

In this paper the author gives the details of an investigation of the orbit of α *Centauri*. Two sets of elements are deduced by him. The following is the second set, which he appears to have derived from equations of condition involving the corrections of the original elements : —

$$T = 1858 \cdot 012$$
$$\varpi = 29\ 33$$
$$\Omega = 177\ 50$$
$$\gamma = 77\ 50$$
$$e = \cdot 966$$
$$n = 4°\cdot 78 \qquad P = 75 \cdot 3 \text{ years}$$
$$= 30'$$

The following is a comparison between the results of these elements and the corresponding results of observation, where P_c denotes the calculated and P_o the observed angle of position; and D_c the calculated and D_o the observed distance.

Date.	P_c ° ′	P_o ° ′	P_c-P_o ′	D_c ″	D_o ″	D_c-D_o ″	Observer.
1826·012	212 23	213 11	—48	20·66	22·45	—1·79	Dunlop
1830·012	214 46	215 2	—16	19·1	19·95	— ·85	Johnson
1834·79	218 15	218 30	—15	16·84	17·4	— ·56	Herschel
1837·34	220 32	220 42	—10	15·48	16·11	— ·63	Herschel
1846·866	235 33	234 18	+75	9·85	9·82	+ ·03	Jacob
1848·023	238 53	237 59	+54	8·61	8·05	+ ·56	Jacob
1850·956	251 13	250 43	+30	6·5	5·97	+ ·53	Jacob
1853·049	266 15	267 34	—79	5·03	4·55	+ ·48	Jacob
1854·003	276 19	276 20	— 1	4·45	4·21	+ ·24	Jacob & Self
1854·632	284 19	283 32	+47	...	...	...	Self

The author remarks that the time of the next periastral passage probably lies between 1857·5 and 1858·5, and that the semi-axis major of the orbit is a little greater than 30″.

The following is a letter from Mr. Hind to Admiral Smyth in reference to the orbit of the same star:—

" I believe a paper upon the elements of α *Centauri* was referred to you for report yesterday. I have recently deduced an orbit for the same star, not being aware that any one else was engaged upon the same investigation. My results, in which I have some confidence, are founded upon all the micrometrical measures from 1834 onwards, including the excellent series by Capt. Jacob down to the present year. I have thought you might like to have the elements for comparison with those given in the communication to the R.A.S. which I have not seen:—

Per. pass. 1859·42

☊ 16° 42′

λ 26 2

Eccentricity 0·7752

Mean annual motion + 4°·448

γ 62° 53′

α 13″·57

Period 80·94 yrs.

" These elements agree very well with the observations, so far as I have examined them.

" *Dec.* 9, 1851."

On an Appearance seen in the Moon.   By Robert Hart, Esq.

(*Letter to the Astronomer Royal.*)

"On the night of the 27th December, 1854, between 6 and 7 P.M., the moon was very bright.   I had brought my 10-inch reflector to bear upon the moon ; on the shaded side of the disk I observed a white spot, where I have marked it on the sketch.   As it was of the colour of the light of the moon, and not like star-light, I thought it part of the moon ; but as it disappeared in less than a minute after I first observed it, I concluded it was a star eclipsed by the moon.*   I now turned my attention to the light part of the disk, and my eye was at once attracted by an appearance I had never seen before on the surface of the moon, although I have observed her often during these last forty years. She was 8^d 4^h old at the time, and just on the edge of the light, where I have marked on the sketch, there were *two luminous spots*, one on either side of a small ridge, *which ridge was in the light*, and of the same colour as the moon ; but these spots were of *a yellow flame colour*, while all the rest of the enlightened part was of a snowy white, and the mountain-tops that were coming into the light, *and just on the shadow side of these spots, were of the same colour as the moon.*   The lights of these spots were *like the light of the setting sun reflected from a window a mile or two off.*   I observed it for five hours.   I thought them rather less bright than as first seen, *but very little less;* so bright were they, when the instrument was the least thing out of focus, they showed *rays around them as a star would do.*

"As I live about two miles out of Glasgow, I had no scientific friend with me at the time, but I called the attention of three gentlemen, my neighbours, and my own household, and they all described the appearance as I saw it myself, and have given above.

"I would have followed it longer had I been able, but the wind was very cold, 20°·5, and I had no shelter, as I take my telescope outside.   I left off about 12 P.M. ; and from the cloudy weather, I never saw the moon until she was nearly full, then only a few minutes, and again on the 10th January, at 4 A.M.; but the haze was too thick.   I thought I might see them in the shade, but I could not : I have never seen her since.

"May I ask the favour of your laying this before the Astronomical Society, that we might learn if any of the members have observed this appearance in the moon, or have heard that it was seen by others.   I shall be happy to learn if it has been taken notice of by any other observer, and their opinion of it.

"It appeared to me, from the brightness of the light and the contrast of colour, *to be two active volcanoes or two mouths of one in action.*

* This was, no doubt, the occultation of μ *Piscium*, which, according to the *Nautical Almanac*, was in conjunction with the moon at 6^h 54^m 10^s, on the evening of December 27, 1854.—EDITOR.

" If there was no star eclipsed by the moon at or about 7 P.M. that night, the white spot that was more like one of the mountain-tops tipped with light than a star, may have had some connexion with it.

" *Cessnock Park, January 15th, 1855.*"

Discovery of a New Comet (*Comet I.* 1855).
*By M. Winnecke.

On the morning of the 15th of January, M. Winnecke dis-covered at Berlin, in the vicinity of γ *Hydræ*, a faint granulous nebula, the estimated position of which was,—

<div align="center">

Jan. 14, 18^h   225° 20'   —27° 11'

</div>

On the next morning it was found to have shifted its place. The following observation of its position was obtained by the discoverer and M. Bruhns:—

	Berlin M.T.	R.A.	Decl.
	h m s	o ' "	o ' "
Jan. 15	18 4 16·2	226 5 15·4	—27 15 5·3

Apparent place of the star of comparison,—

	R.A.	Decl.
	o ' "	o ' "
Argel Z 373, No. 57	226 38 16	—27 18 42·4

The daily motion is consequently,—

<div align="center">

In R.A. about   +45'

Dec. —   — 4'

</div>

The comet has also been discovered independently by M. Dien, at the Imperial Observatory, Paris.

M. Bruhns has calculated an ephemeris of the comet, extend-ing from Jan. 14 to Feb. 17, printed copies of which have been circulated by M. Peters, the editor of the *Astronomische Nach-richten.*

The weather has hitherto been very unfavourable for observing this comet in England.

On the Orbit of the Binary Star σ Coronæ Borealis.
By Eyre B. Powell, Esq.

" Three orbits have been already arrived at for this star, viz. that computed by Sir John Herschel, and the approximately inter-accordant ones of Messrs. Hind and Mädler; it may, therefore, appear, at first sight, a work of supererogation to investigate a fresh set of elements. The startling difference, however, that exists between Sir J. Herschel's orbit and the other two, will,

perhaps, be held to excuse my troubling the Society with the present communication.

"By the graphical method I obtained the following results for the apparent orbit of σ.

$$a = 2''·725$$
$$e = ·6479$$

Minimum distance	$= 1''·27$
Position for do.	$= 91° 20'$
Maximum distance	$= 2''·74$
Position for do.	$= 324° 30'$

"The apparent ellipse then gave the elements of the real orbit as follow:—

$$τ = 1829·7$$
$$ϖ = 102° 50'$$
$$☊ = 3\ 8$$
$$γ = 45\ 6; λ = 96° 53'$$
$$e = ·3887$$
$$n = + 1°·5, \text{ Period } = 240 \text{ years.}$$
$$d = 2''·94$$

"I annex two tables, one affording a comparison between the results of computation and those of observation for angles of position, the other effecting the same object for distances.

Comparison of Calculated and Observed Positions.

Date.	$θ_c$	$θ_o$	$θ_c - θ_o$	Observers.
	° ′	° ′	° ′	
1780·6	347 48	347 32	+ 16	♅ sec. *Cycle**
1781·79	348 57	347 32	+1 25	♅ sec. H
1802·74	13 58	11 24	+2 34	♅
1822·48	66 44	66 24	+ 20	H & S, Σ†
1824·45	75 57	75 10	+ 47	H & S, S‡
1826·77	87 37	89 0	−1 23	Σ
1830·52	107 3	106 51	+ 12	H, D, Sm§
1831·34	111 9	111 32′	− 23	D
1832·459	116 37 -	115 26	+1 11	D, Sm‖
1833·47	121 20	120 39	+ 41	D, Sm¶
1835·5	130 11	130 54	− 43	Sm
1839·67	145 27	145 6	+ 21	Sm
1843·35	156 8	155 54	+ 14	Sm
1846·22	163 6	163 45	− 39	J
1853·513	177 7	176 59	+ 8	J & Self**

* I believe the *Cycle* follows the *Philosoph. Trans.* See vol. v. Astron. Soc. *Memoirs.*

† 1821·3 65° 15′ H & S; 1822·67 61° 0′ Σ; 1823·47 72° 56′ H & S.

‡ 1823·47 72° 56′ H & S; 1825·44 77° 31′ S.

§ 1830·28 105° 5′ H; 1830·52 107° 17′ D; 1830·76 107° 36′ Sm.

‖ 1832·549 115° 57′ D; 1832·37 114° 54′ Sm.

¶ 1833·36 120° 37′ D; 1833·58 120° 42′ Sm.

** 1853·142 177° 54′ J; 1853·348 175° 12′ Self; 1854·048 177° 52′ J.

Comparison of Calculated and Observed Distances.

Date.	d_c	d_o	$d_c - d_o$	Observers.	Remarks.
1823·47	1·34	1·45	−·11	H & S	
1826·77	1·28	1·3	−·02	Ƶ	
1830·52	1·29	1·26	+·03	H, Sm	1830·28, 1″·22 H ;
1832·37	1·33	1·4	−·07	Sm	1830·76, 1″·3 Sm
1833·47	1·36	1·25	+·11	D, Sm	1833·36, 1‴·3 D ;
1835·5	1·43	1·4	+·03	Sm	1833·58, 1″·2 Sm
1839·67	1·62	1·6	+·02	Sm	
1843·35	1·7975	1·8	−·0025	Sm	
1846·22	1·94	1·87	+·07	J	
1853·7	2·25	2·21	+·04	J	

♅ stands for Sir W. Herschel ; H for Sir J. Herschel ; Ƶ for Prof. Struve ; S for Sir J. South ; Sm for Admiral Smyth ; D for the Rev. Mr. Dawes ; J for Captain Jacob.

"It is not necessary to dwell upon the foregoing ; but I would remark that my orbit approaches much nearer to the early one of Sir J. Herschel than it does to those computed by Messrs. Mädler and Hind at more recent dates. So far as I am able to judge, there does not appear to be ground for believing the period to extend over six or seven hundred years.

"I did not think it worth while to form equations of condition with the view of correcting the elements, as the orbit represents the observations, more especially those of distance, with very considerable accuracy : indeed in no other case have I arrived at equally respectable results, I will not say after the first, but even after the third or fourth approximation.

"*Madras, Nov. 20th, 1854.*"

Observations of the Zodiacal Light in 1854.
By T. W. Burr, Esq.

In continuation of my previous notice of the zodiacal light, I have to report that in the spring of 1854 I observed it on the evenings of February 16th, 18th, 23d, 25th, and 26th ; March 17th, 21st, 23d, 27th, and 30th ; and April 17th. The first and two last appearances were mere glimpses, but on February 18th the view was exceedingly good ; and those of 21st March from 8ʰ 10ᵐ to 9ʰ Greenwich mean time, and 23d March from 8ʰ to 8ʰ 30ᵐ, were as fine as possible in this latitude and locality.

It is unnecessary to describe its course and appearance with any great minuteness, as there is always a difficulty in ascertaining its exact extent. Generally it very much resembled the description I gave of it in the preceding year, extending in the early

observations to the vicinity of the Pleiades, and in the later ones to that of *Aldebaran.* Upon the whole its boundaries were not so well defined as in 1853.

I have observed occasional sudden variations in its brightness, but have been unable to detect anything like a periodicity in these changes; and knowing the great influence of atmospheric causes on objects having low altitude, I should hesitate to ascribe them to a real variation in the brightness of the light. Generally speaking, when best seen, the brilliancy was greater than that of the milky way, which was usually visible at the same time in the north-west.

Highbury, January 1855.

On the Pendulum Experiment for Illustrating the Rotation of the Earth. By Lieut. Ashe, R.N.

After some geometrical illustrations, the author gives the following description of the pendulum employed by him in these experiments:—

"In the centre of an equilateral plate a small hole was drilled, through which a piece of the best pianoforte wire, a foot in length, was put, fitting tight, and secured to the plate by the wire having a nut screwed on the end of it, and a loop was turned at the other end of the wire. To this steel circular spring a copper wire, stretched and rubbed, was attached; the length of pendulum was 52 feet, and the weight, a leaden sphere, 17 lbs. The plate was firmly screwed to a beam in the steeple of Chalmers' Church of this city (Quebec); the pendulum was protected from any current of air. At about the height of the eye, when sitting down, a wooden circle, 4 inches broad, 3 inches thick, and 8 feet in diameter, was fixed and graduated on the inner edge, so that by placing the eye on a level with the circumference, the time of the wire coinciding with each degree could be noted with great nicety.

"I found steel wire more unsatisfactory in its results than any other sort, in consequence of its being so very sonorous,—so much so, that the noise of a cricket, or any other sound that would harmonise with the wire, produced undulations."

Description of Shepherd's Galvano-Magnetic Regulator. By Henry S. Ellis.

(*Communicated by Dr. Lee.*)

I beg to forward a description of the galvano-magnetic regulator and apparatus, which have been manufactured by the

patentee, Mr. C. Shepherd, of Leadenhall Street, and placed in the Exeter Guildhall, for the purpose of giving hour-signals to the cathedral clock, and denoting uniform time throughout this city. In further explanation of the same I enclose three drawings.*

No. 1 is a general outline of the regulator, and is very similar to the one at the Greenwich Observatory, except that there is no contact-spring in connexion with the hour-wheel, which was found to be unnecessary.

No. 2 is the dial of the regulator, and shows the contact-springs for giving hour-signals to the cathedral clock. The lower pair of springs (A) is brought into contact with each other every hour, when the pin (B) in the minute-wheel presses them together; and similarly the upper pair of springs (C) are brought into contact every minute by the pin (D) in the stud underneath the seconds hand; but the whole circuit, between the batteries and the magnets at the cathedral, is not completed until both the lower and the upper pairs of springs make contact simultaneously; and as seven seconds elapse between the signal and the first blow struck on the bell, the contacts are made in anticipation of the hour, so that the striking shall coincide exactly with time shown by the regulator.

No. 3 is the old striking part of the cathedral clock, and the new galvano-magnetic apparatus which is placed in electric connexion with the regulator. When the hour-signal is given, the armature (A) is drawn down on the poles of the magnets (B B), the perpendicular to it is brought forward, and the hammer (D) falls on the tail of detent (E); the stop-piece (F F) is then released, and the striking train performs its work. The pin-wheel (G) raises the hammer (D) again into its horizontal position in readiness for the next hour.

The whole of the iron work in the new apparatus was made much heavier than is absolutely necessary. At first twenty-four of Smee's batteries were requisite to draw the armatures to the magnets; but by lessening the weight of the hammer (C) and the counterpoise (H), it has been found that nine of the same batteries are sufficient—although, to guard against a want of power, and to prevent the necessity of frequent charging, a larger number is now applied. Other parts might be made considerably lighter, and were that done probably about six batteries would be sufficient.

It may be remarked, that if a simple galvanometer could be attached to show the waste of battery power it would be a great convenience.

It is proposed to place a large electric dial, or dials, on the exterior of the Guildhall, and some of the distant parishes have it in contemplation to purchase new clocks, and to put them in connexion with the Guildhall regulator. Indeed, I believe it would

* See Illustrations at the end of this Notice.

be practicable to connect all the existing old clocks and dials with it, at a trifling cost beyond that of laying down the electric wires.

When the Electric Telegraph Company bring their wires into the city, it is hoped that they will give the Exonians the benefit of one of the Astronomer Royal's daily signals—the deflection of a needle in immediate proximity to the regulator would be sufficient. For the present a 12-inch sympathetic dial of Shepherd's in my own observatory reports to me the performance of the regulator; and there will be little difficulty in making all the public clocks indicate and strike Greenwich mean time, within a second, throughout the year.

In conclusion, I have to acknowledge, with my very best thanks, the readiness with which the Dean and Chapter permitted the cathedral clock to be connected with the Guildhall regulator; and also the cordial support received from the Right Worshipful the Mayor, the Town Council, and others interested in the carrying out of these chronometrical arrangements.

Exeter, November 8, 1854.

Observations of the Solar Spots. By Dr. Wolf.

1854.	Days of Observation.	Days free of Spots.	Number of new Groups in the Month.
January	23	4	4
February	22	6	4
March	29	6	7
April	25	2	6
May	28	7	7
June	30	1	9
July	29	4	7
August	30	4	4
September	30	5	7
October	24	8	5
November	16	0	4
December	18	4	4
	304	51	68

The following table exhibits a synopsis of the condition of the sun during the last six years, in regard to the relative mean number of groups of spots visible on his disk in each month of the year. In order to take some account of the magnitude of the groups, the results are increased by one-tenth of the number of spots visible.

	1849.	1850.	1851.	1852.	1853.	1854.
Jan.	17·3	10·3	9·0	7·6	4·2	1·4
Feb.	14·3	10·0	10·0	6·3	5·1	1·7
March	10·1	8·8	6·8	6·7	3·3	1·9
April	11·5	3·6	6·1	6·7	5·0	3·1
May	9·6	5·4	7·0	5·4	3·5	2·2
June	9·8	10·5	6·5	4·8	5·0	2·0
July	9·0	4·8	3·1	4·4	4·7	1·9
Aug.	7·0	6·7	6·2	4·4	5·3	1·6
Sept.	10·1	9·1	7·4	3·6	3·1	2·4
Oct.	9·0	9·2	6·3	10·2	·4·5	1·7
Nov.	11·9	4·8	6·1	6·7	2·8	2·0
Dec.	10·9	5·2	6·8	5·3	2·6	1·5
Sum	130·7	88·4	81·3	72·1	49·1	23·4

These results seem to agree with the period of the solar spots which Dr. Wolf has assigned, according to which the minimum of the number of groups falls in the latter half of the year 1855.

Dr. Drew has for sale a 30-inch transit-circle by the late Mr. Jones, of Charing Cross. The telescope is of $3\frac{1}{2}$-feet focal length, with an object-glass of $3\frac{1}{4}$ inches diameter. The circle is read off by three microscopes mounted on solid stone arms; is provided with a micrometer eye-piece and every means for the nicest adjustment. The collimation-error may be corrected either by reflection or by the aid of two collimators which accompany the instrument. Price, with the stone mounting, 100*l.*

A drawing and description of the instrument will be forwarded by Mr. Williams, the Assistant-Secretary of the Society, to any person applying for the same.

ERRATA.

The reader is requested to correct the following errata, which were committed in the translation of Professor Hansen's paper on the Construction of New Lunar Tables, &c., inserted in the *Monthly Notices* for November last :—

Page 12, line 8 from bottom, *for* a large number, *read* a moderate number.

— 14, line 1, *for* were different, *read* were not different.

— 14, last line, *for* greater than unity, *read* less than unity (kleiner wie Eins).

N.º 3.

CONTENTS.

THE Annual General Meeting of the Society, G. B. AIRY, Esq., President, in the Chair.

Frederick Brodie, Esq., The Gore, Eastbourne, Sussex;
H. S. Ellis, Esq., Exeter;
William Lethbridge, Esq., St. Paul's School;
H. W. Buxton, Esq., 37 Abbey Road, Regent's Park;
Jon. T. Owen, Esq., Swansea; and
Charles H. Wild, Esq., 103 St. Martin's Lane,

were balloted for and duly elected Fellows of the Society.

Report of the Council to the Thirty-fifth Annual General Meeting of the Society.

The Council, in presenting the following Report, desire to congratulate their constituents on the state of the Society, and on the general progress of the science of astronomy.

The Report of the Auditors, subjoined, will show the state of the finances :—

RECEIPTS.

	£	s.	d.
Balance of last year's account	458	3	11
By dividend on £2619 5s. 9d. 3¼ per Cents	41	6	6
By ditto on £1650 Consols..	23	6	2
By ditto on £2832 16s. 11d. 3¼ per Cents	43	7	0
By ditto on £1650 Consols.	23	6	2
On account of arrears of contributions	55	14	0
104 contributions (1854–55)	218	8	0
4 ditto (1855–56)	8	8	0
12 compositions	252	0	0
24 admission fees.	50	8	0
15 first year's contributions	26	5	0
Sale of Publications	59	10	6
	£1260	3	3

EXPENDITURE.

	£	s.	d.
Cash paid Mr. Basire, engraver	18	4	6
J. Rumfitt, bookbinder	9	0	4
Mr. R. Grant	40	0	0
George Barclay, printer	114	0	8
Mrs. Jones (Lee Fund)	3	5	6
Mr. R. Grant	10	10	0
Investing compositions of E. B. Powell, C. G. Prideaux, A. B. Martin, T. W. Burr, James Cockle, W. H. Besant, and W. Huggins, Esqs., and Capt. Toynbee	189	0	0
J. Rumfitt	8	15	0
Mr. R. Grant	10	0	0
Investing compositions of James Samuel and J. J. Burman, Esqs.	42	0	0
James Basire, engraver	22	6	0
Mr. R. Grant	10	0	0
George Barclay	208	17	6
J. Rumfitt	9	15	11

Taxes { 1 year's land tax 5 12 6
      { 1 year's property tax 1 9 2

	£	s.	d.
	7	1	8
J. Williams' salary	100	0	0
Ditto commission on collecting £418 14s. 6d..........	20	18	6
Charges on books, and carriage of parcels	3	15	2
Postage of letters and Monthly Notices	36	15	9
Porter's and charwoman's work	24	14	4
Tea, sugar, biscuits, &c. for evening meetings	13	13	0
Coals, candles, &c.	15	6	6
Waiters attending meetings	3	17	0
Sundry disbursements by the Treasurer	23	14	3
Balance in the hands of the Treasurer	314	11	8
	£1260	3	3

Assets and present property of the Society : —

	£	s.	d.
Balance in the Treasurer's hands	314	11	8
1 contribution of 7 years' standing 14 14 0			
4 ——— of 6 ditto 50 8 0			
3 ——— of 5 ditto 31 10 0			
3 ——— of 4 ditto 25 4 0			
5 ——— of 3 ditto 31 10 0			
16 ——— of 2 ditto 67 4 0			
18 ——— of 1 ditto 37 16 0			
	258	6	0
Due for publications of the Society	12	12	6

£1650 3 per Cent Consols.
£2832 16s. 11d. 3¼ per Cent Annuities.
Unsold publications of the Society.
Various astronomical instruments, books, prints, &c.

	£	s.	d.
The balance of the Turnor Fund (included in Treasurer's balance above)...	24	8	6

Stock of volumes of the *Memoirs :* —

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	39	VII.	221	XVII.	247
I. Part 2	82	VIII.	208	XVIII.	258
II. Part 1	100	IX.	214	XIX.	273
II. Part 2	62	X.	226	XX.	272
III. Part 1	129	XI.	236	XXI. Part 1 (separate).	333
III. Part 2	149	XII.	243	XXI. Part 2 (separate).	62
IV. Part 1	151	XIII.	262	XXI. (together).	210
IV. Part 2	164	XIV.	447		
V.	178	XV.	275	XXII.	251
VI.	198	XVI.	262	XXIII.	444

Progress and present state of the Society : —

	Compounders.	Annual Contributors.	Non-residents.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1854	140	182	64	6	392	58	450
Since elected	11	13	...	...	24	4	...
Deceased	—2	—4	—3	...	—9	—2	...
Removals	2	—2	...	...	...	...	...
Resigned	...	—3	...	...	—3	...	...
February 1855	151	186	61	6	404	60	464

The instruments belonging to the Society are now distributed
's follows : —

> The *Harrison* clock,
> The *Owen* portable circle,
> The *Owen* portable quadruple sextant,
> The *Beaufoy* circle,
> The *Beaufoy* clock,
> The *Herschelian* 7-foot reflector,
> The *Greig* universal instrument,
> The *Smeaton* equatorial,
> The *Cavendish* apparatus,
> The *Lee* circle,
> The 7-foot Gregorian telescope (late Mr. Shearman's),
> The Universal quadrant by Abraham Sharp,
> The *Fuller* theodolite,

are in the apartments of the Society.

The Brass quadrant, said to have been *Lacaille's*,
is in the apartments of the Royal Society.

The Standard scale
is in the charge of the Astronomer Royal, with the consent of the
Council, to be employed in the construction of a new Standard
Measure, under the direction of the Standard Committee.

The *Wollaston* telescope,
having been returned by the representatives of the late Professor
Schumacher, is now in the hands of Mr. Dollond for repair.

The remaining instruments are lent, during the pleasure of the
Council, to the several parties under mentioned, viz.:—

> The other *Beaufoy* clock,   ⎱ to the Royal Society.
> The two invariable pendulums, ⎰
> The Variation transit (late Mr. Shearman's), to Mr.
>   Gravatt.

Among the presents will be found some valuable editions of
different writings of Galileo, presented by Sir W. C. Trevelyan,
Bart. In thus thanking the donor for his very acceptable con-
tribution to our library, the Council remark that a doubt seems to
have existed as to whether it would be worth while to offer the
works to a library professedly astronomical, and which, therefore,
might be presumed to possess copies. It should be known that,
up to the present time, our library is by no means rich in anything
but the astronomy of the present century. The great bulk of it,
with the exception of the books of the Mathematical Society,
consists of presents of the works of men who have lived in our
own day. The application of the Turnor Fund will surely, though
very slowly, remedy this defect: in the meantime, works of the
astronomers of the three centuries preceding the present one will
be valuable additions, and will be duly appreciated.

The medal has been awarded to the Rev. W. R. Dawes, for his
astronomical labours generally, but especially for those recorded in
our *Memoirs*. The President will, in the usual manner, lay before
the Society the grounds of this award, and present the medal, at
the conclusion of the ordinary business.

The twenty-third volume of the *Memoirs* has been published
some time since. The Council would direct the attention of the
Fellows of the Society to a Paper in it by Lord Wrottesley, con-
taining a Catalogue of the right ascensions of somewhat more than
one thousand stars. This is not the first occasion on which the
Council have had the pleasure of directing attention to the labours
of this excellent observer. From the recent researches of various
astronomers on the motion of the solar system in space, and the

remarkably accordant results which have been deduced from such inquiries, the determination of the places of the stars acquires a high degree of importance in a speculative point of view, independent of its otherwise inestimable value in forming a sure groundwork upon which to establish the fundamental points of astronomy.

The same volume of the *Memoirs* also contains some results of Mr. Lassell's observations at Malta, to which allusion was made in the last Report of the Council.

The *Monthly Notices* still continue to be conducted upon the plan introduced by Mr. Sheepshanks about two years since, and to which allusion was made in the last Report of the Council. It may now be stated pretty confidently that this modification of the original plan has answered the purpose for which it was designed, and appears to have given general satisfaction to the Fellows of the Society. Indeed, it could hardly be doubted, that by adapting the form of publication so as to co-operate with the *Astronomische Nachrichten*, instead of clashing with the arrangements of that valuable journal, the *Monthly Notices* would assume a more special character, and their utility in promoting the interests of astronomical science would thereby be materially enhanced. It may, perhaps, be unnecessary to repeat, that although it has been deemed expedient to assign to the foreign periodical just referred to the publication of all ephemerides and observations of the minor planets, as being the channel best adapted for such communications, still there remains an immense field of observation and research, in respect to which the pages of the *Monthly Notices* offer a readily accessible medium of intercourse among astronomers. If there is one point more than another which the Council would urge upon the Fellows of the Society in regard to promoting the usefulness of the *Monthly Notices*, it is the importance of subjecting to a searching process of computation the numerous and valuable results of observation which emanate annually from the public observatories of the country, and also occasionally from several private observatories. With respect to observations of an exceptional character, such as those relating to comets, double stars, and satellites, it is gratifying to find that an increasing desire has recently been evinced to apply the rich store of observations which have been accumulated in recent years towards improving some of the elements of astronomy. It cannot be expected that those more extensive series of observations constituting the normal results established in continuous succession at the public observatories of the country will so frequently form the subject of discussion. Still, the Council cannot refrain from reminding the Fellows of the Society that the facilities for such researches have been very much increased in recent years. From the circumstance of the results of the Greenwich observations being now published annually in a separate form, which may be obtained gratuitously upon application at the Royal Observatory, a confident hope is enter-

tained that our countrymen will not be slow to avail themselves of those precious materials.

It will have been remarked by the Fellows of the Society, that since the commencement of the present session the *Monthly Notices* have appeared upon a stamped sheet. In regard to the easier transmission of the *Notices* to their various destinations, the practicability of employing a better quality of paper than hitherto, as well as several other obvious advantages, the expediency of this alteration cannot fail to suggest itself to every one. It is to be hoped, more especially, that it will have the effect of facilitating the transmission of the *Notices* to various countries on the Continent, where, hitherto, serious impediments have existed in regard to this object.

Our obituary list for the past year contains the names of Baron von Lindenau, Dr. Petersen, and M. Mauvais, Associates, and of the following Fellows:—Captain Blackwood; Edward Riddle, Esq.; Lieut. St. John; Professor Scott; Robert Snow, Esq.; the Rev. J. B. Wildig; and Dr. Whittaker. The official recognition of the loss of the Arctic expedition has made it the duty of the Council to add to the list of this year the names of Sir John Franklin and Captain Crozier.

BERNARD VON LINDENAU was born in Altenburg, the capital of Saxe Altenburg, on the 11th June, 1780. His father was a wealthy proprietor. He studied law and political economy at Leipsic, but even there gave his leisure hours to his favourite pursuits, mathematics and astronomy.

On the completion of his legal studies, Von Lindenau repaired to the Observatory of Seeberg, where he continued his astronomical education under the direction of Von Zach. His progress was proportioned to his talents and unwearied industry, and when Von Zach left Seeberg in 1804, the direction of the Observatory and editorship of the celebrated *Monatliche Correspondenz*, which was established by Von Zach, were both committed to him.

In 1814, Von Lindenau left the Observatory in Nikolai's charge, while he made the campaign as adjutant to the Duke of Saxe Weimar. After the war, though he returned to the Observatory, and did not altogether neglect astronomy, his attention was directed to government and administration. The capacity with which he conducted the business intrusted to him, and the services which he rendered to Saxe Altenburg and Saxe Gotha, at that time united, raised him in a few years to the first offices in the State. He was prime minister of the two dukedoms, while he still retained, under the title of curator, the supervision of the affairs of the Observatory and of its working.

When the reigning line of Gotha and Altenburg failed, and those possessions were united to Saxe Coburg, Von Lindenau entered into the service of the King of Saxony, and was for some years the representative of Saxony at the Frankfort Diet. He

then returned to Dresden as Minister of the Interior, an office which he actively discharged till 1843. In this year he took leave of public life, and spent the remainder of his days at Altenburg on his own property. Although his important ministerial duties forbade, during a large portion of his life, much personal contribution to astronomy, he never lost sight of this science. He took a lively interest in everything which occurred, and kept up as active a correspondence with his astronomical friends, Bessel, Gauss, Schumacher, Hansen, Olbers, Von Zach, Encke, &c., as his public occupations permitted.

On his retirement, Von Lindenau gave himself up to science and art, and resumed his original activity. He executed scientific researches, completed his rich library, and built a museum, which he furnished with the treasures he had already purchased of ancient and modern art.

Providence kindly favoured him with almost uninterrupted good health, till within a few weeks of his decease.

He died on the 21st May, 1854, at ten in the morning, possessing his intellects unclouded to the last.

After this sketch of Von Lindenau's scientific and public services, it should be added that he is to be classed among those men in whom great intellectual powers are united with the Christian virtues. A manly uprightness, which knew no way but that which is straightforward, guided him in all his dealings, and his sole desire was to act according to his convictions. With this he combined great benevolence. A large part of the income which he derived from several inheritances was devoted to charitable purposes, and he had the happiness to alleviate many a want and to dry many a tear. He bequeathed nearly half of his fortune to public or charitable purposes. His library and museum, with the means for supporting them, were left to Altenburg, 60,000 thalers to the charitable institutions of Altenburg, and 30,000 to those of Gotha, besides legacies to particular persons. By his will he bequeathed his chronometers to his scientific friends; the chronometer presented to him by Frederic VI. of Denmark to Professor Hansen, and two by Emery to Professors Encke and Möbius, with a request that they should descend in their families as a remembrance of him. It may truly be said of Bernard von Lindenau that his memory will live not only in the results of his industry and talent but in his Christian virtues.

ADOLPHUS CORNELIUS PETERSEN was born July 23, 1804, at Vester-Bau, in Schleswig, where his father was a farmer. Up

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* Professor Schumacher used to tell with great humour a story of Von Lindenau, which marks well his chivalrous character. When the Allies had entered Paris, one of his fellow-officers, congratulating him on their success, applied a term of insult to Napoleon. "We Germans may hate Napoleon," was Von Lindenau's reply, "but not insult him." The dispute grew so warm, that it ended in a duel, in which Von Lindenau received a wound, that afterwards caused him much trouble and a painful operation.

to the time of his confirmation, in March 1820, he attended an elementary school at Buhrkarl, and occasionally assisted his father in husbandry. He subsequently learned land-surveying, and got so well acquainted with the subject in a year, that he was able to undertake the execution of plans without assistance. From 1820 to 1824 he was partly employed in measuring, and partly in plan-drawing. In the spring of 1825 he was engaged on the drainage of the district of Tondern, which had suffered greatly from the floods of the 3d and 4th of February. Here he got acquainted with the late Captain Von Caroc, one of the ablest officers em-ployed in the measurement of the Danish arc of the meridian; and through him Petersen became known to Professor Schumacher, who invited him to fill the place of Thomas Clausen, as assistant-observer at the Altona Observatory (Clausen had been invited to Munich by the optical establishment of Utzschneider). In this situation he carried on the current observations of the Altona Observatory with great zeal and skill for twenty-four years, till Schumacher's death, and distinguished himself by the discovery of three comets. At the same time he took a share in many other scientific labours, which we owe to the Altona Observatory. In 1829 and 1830 he made the astronomical observations for the pendulum experiments at Güldenstein, and afterwards worked for many years on Schumacher's arc of the meridian. He connected the Amager base, measured with Bessel's apparatus, with the triangulation. He also connected Schumacher's triangulation with that of Sweden, and took a considerable share in its connexion with that of Prussia.

Bessel has given a striking proof of his reliance on Petersen's scrupulous trustworthiness. This great astronomer, shortly before his death, specially directed that Petersen and Busch should com-plete the reduction of the declinations of the fundamental fixed stars, which he had observed with the Repsold Meridian Circle, and in the way in which he himself had already commenced the work. In consequence of this honourable commission, Petersen visited Königsberg to bring away the necessary papers. The Philosophical Faculty of Königsberg took this opportunity of pre-senting him with a doctor's diploma. These reductions were com-pleted before Petersen's death, and the publication may be looked for from Professor Busch, the director of the Königsberg Ob-servatory, at no very distant time.

After Schumacher's death, Petersen held, temporarily, the di-rection of the Altona Observatory, and continued the publication of the *Astronomische Nachrichten*, at first alone, but afterwards in conjunction with Professor Hansen. In 1852 he was named by the King of Denmark a member of the commission for regulating the Altona Observatory, and received the title of Professor. He died at Altona on the 3d of February, 1854.

Petersen, as is evident from the preceding notice, came to Altona with but little knowledge of mathematics or astronomy; but he strove with great zeal and great success to repair these

deficiencies. The volumes of the *Nachrichten*, from the 7th to
the 37th, give abundant evidence of his astronomical proficiency,
especially in the departments of observation and calculation. He
married Sept. 2, 1832, but left no family. His private life was
in the highest degree peaceful and happy, and his upright and
unpretending character won the respect of all who knew him.

FELIX-VICTOR MAUVAIS was born at Maiche, a village in the
Department of Doubs, France, on the 7th March, 1809.

He was sent early to the seminary of Besançon, and there laid
the foundation of a good education. The desire of acquiring more
extended knowledge induced him to proceed to Paris, where, after
pursuing his studies with assiduity in the various schools of
learning of the capital, he was admitted as a student at the Ecole
Polytechnique. Monsieur Mauvais afterwards gave instruction in
the Mathematics at the Institution of Monsieur Barbet.

About this time he received a gratifying mark of the esteem
in which he was held in his native Department by being com-
prised in the number of young men adopted by the town of
Besançon, when he received a pension on the Suard Foundation,
which afforded him additional means for pursuing his studies.

His taste for astronomy and for the exact sciences generally
rendered him desirous of being attached to the Observatory, to
which he was appointed by the Bureau des Longitudes in 1836, at
the recommendation of Monsieur Arago.

Finding himself permanently attached to the Observatory, and
at liberty to follow his favourite scientific pursuits, he did not
limit his labours to an active participation in the ordinary duties
of the establishment, but entered with ardour upon various astro-
nomical investigations.

His zealous exertions were rewarded by the discovery of four
comets.

During the eighteen years that Mons. Mauvais belonged to the
Observatory he was indefatigable in making observations, and
acquired much distinction as a practical astronomer, so that his
labours added much to the high reputation of that establishment.

The talents and industry displayed by Mons. Mauvais caused
him to be appointed a member of the Institute of France. He
was named in 1843 to succeed Mons. Bouvard in that learned
body; and at the end of the same year he was selected as an
astronomer to the Bureau des Longitudes.

His varied acquirements and his thorough acquaintance with
every part of practical astronomy caused his opinions to be much
valued at the meetings of this body. Mons. Mauvais was not less
distinguished for his amiability and independence of character
than for his scientific attainments.

His health began to fail under his continual labours: however,
his love of science was too great to allow him to seek renewed
health in tranquillity and a cessation from astronomical studies.

This distinguished astronomer died in March 1854, at the com-

paratively early age of forty-five, much regretted by his numerous friends and by those who looked forward to further contributions to science from so intelligent and so energetic an astronomer.

Capt. FRANCIS PRICE BLACKWOOD, R.N., was the second son of Vice-Admiral the Hon. Sir H. Blackwood. He entered the Navy in 1821, at the age of thirteen, and was promoted to the rank of captain in 1836. His activity and his taste for scientific pursuits pointed him out as well qualified to conduct the examination of part of the north-east coast of Australia and the adjacent seas, which were imperfectly known; and in the beginning of 1842 he sailed in command of H. M. S. Fly on that service. While visiting the island of Teneriffe he ascended the Peak and obtained a satisfactory barometric observation, which afforded a height agreeing very nearly with that of Humboldt: he also made a slight survey of the island of St. Paul's, in the Indian Ocean, which was published by the Admiralty. Capt. Blackwood made a detailed examination of many of the small groups of islands within the barrier reefs on the north-east coast of Australia, and of the " inner passage," or expanse of smooth water between these reefs and the coast.

This service, performed amongst coral reefs rising suddenly from great depths, was one of considerable danger, and proved that Capt. Blackwood possessed in an eminent degree the professional skill and personal qualities which are indispensable to a successful issue in such circumstances, and by which, combined with patience and good temper, he preserved his crew from hostile collision, except on two occasions, with the treacherous natives of Australia and New Guinea. Capt. Blackwood erected a beacon on Caine's Island,—a very important position to navigators in those seas.

Some occultations observed by Capt. Blackwood and computed by Mr. Breen will be found in the *Monthly Notices* of the Society. He also devoted particular attention to the measurement of some important differences of longitude.

An account of the voyage was published by Mr. B. Jukes, M.A., the naturalist of the expedition, by the authority of the Admiralty, under the title of *Narrative of the Surveying Voyage of H.M.S. Fly.*

Capt. Blackwood returned after this expedition to the ordinary course of service; but his career, on which his zeal and abilities would, it was hoped, confer lustre, was prematurely closed by cancer, of which he died, at the age of forty-five, much regretted. He has left a widow and two children.

EDWARD RIDDLE was born in 1788, at Troughend, in the county of Northumberland. At this place he received his early education, and afterwards attended a school at Otterburn, on Reedwater, a village not less interesting from its beautiful and romantic situation than from its historical reminiscences. At this ancient

village Mr. Riddle commenced his professional career, and kept a school for a short period. Here he became acquainted with a very remarkable person, the late Mr. James Thompson, who, from his varied attainments in science and natural philosophy, was regarded as a "prodigy" by the rustic population of the village and neighbourhood. In his intercourse with this gifted man, it is not improbable that Mr. Riddle caught a spark of that enthusiasm for science which pervaded his character, and derived from him that taste for scientific pursuits which he retained to the end of his life. It is well known that he urgently pressed upon Mr. Riddle the absolute necessity of acquiring a thorough knowledge of the principles of geometry, and to take Playfair's *Euclid* as his guide, assuring him that it was "a capital book."

While at Otterburn, Mr. Riddle made an electrical machine with his own hands, and with it showed all the ordinary phenomena produced by that instrument : and at that period it may be easily imagined what wonder and alarm would fill the minds of a row of rustics when the electric impulse was made to dart through their bodies with a sensation never before experienced.

In 1807 Mr. Riddle removed from Otterburn to Whitburn, in the county of Durham, where he remained nearly seven years. Here he commenced to study science in earnest ; and, acting on the wise counsel given him by Mr. Thompson of Otterburn, he first completely mastered the whole of Playfair's *Euclid;* and he has been heard to assert that the accomplishment of this task produced such an effect upon his mind as to render the acquisition of any other mathematical subject a matter of comparative ease. He now became a *Diary* correspondent, his first communications to that work appearing in 1810, dated from Whitburn. This periodical was at that time under the management of Dr. Hutton ; and Mr. Riddle's mathematical correspondence speedily procured him the esteem and friendship of that distinguished mathematician, who rendered him important assistance in advancing his success in life. Though, at this period, Mr. Riddle was little more than twenty years of age, he had acquired a considerable knowledge of mathematical science, and for many years he continued to be a distinguished contributor to the *Diary*, in which his solutions were always remarkable for conciseness, elegance, and accuracy. In the years 1814 and 1819 Mr. Riddle obtained the prize given by the editor of that well-known periodical.

Through the recommendation of his friend, Dr. Hutton, Mr. Riddle was appointed, in 1814, Master of the Trinity House School, Newcastle-upon-Tyne. The nautical instruction in this school, at the time of Mr. Riddle's appointment, was in the lowest possible state ; but, by his zeal and abilities, he speedily raised its character to a high standard. His fame, as a teacher of navigation and nautical astronomy, spread far and wide ; he drew around him a large number of pupils, and gained the respect and esteem of a wide circle of friends and contemporary teachers, who, after he had left Newcastle, never ceased to remember him with feelings

of the deepest regard. Here also he became noted for the sur-
prising quickness and accuracy with which he took celestial
observations.

In 1821, Mr. Riddle published an essay, entitled, " Observations
on the Present State of Nautical Astronomy ; with Remarks on
the Expediency of Promoting a more General Acquaintance with
the Modern Improvements in the Science among the Seamen in
the British Merchant Service." This essay, which was dedicated
to the Master and Brethren of the Trinity House, Newcastle, is
admirably written, and reflects great credit on its author. From
a series of lunar observations, taken in 1821, and recorded in this
essay, the longitude of Trinity House School was found to be
1° 37' 17" W.

In 1821, by the same powerful influence of Dr. Hutton, Mr.
Riddle was appointed Master of the Mathematical School, Royal
Naval Hospital, Greenwich, where he remained to the time of his
retirement in 1851. Soon after his removal to Greenwich he
was elected a Fellow of this Society, and, in 1825, his name first
appeared on the list of Council for that year. From that period to
1851 he was either one of the Council or a vice-president, and
took an active part in every plan of the Society for the advance-
ment of astronomical science. Mr. Riddle was always a regular
attendant at the meetings of Council ; and it is remarkable that
he never attended one of these meetings without having previously
obtained leave of absence from the Governor of the Institution.
He was always so highly esteemed by the authorities of Green-
wich Hospital, that he might have left his post on these occasions
without the fear of censure, but his stern integrity, and unbending
rectitude of character, would not suffer him to neglect the monthly
presentation of his request ; and it is needless to say that such
leave of absence was never refused him.

Mr. Riddle contributed some valuable practical papers to the
Memoirs of this Society. Among them is one " On the Longi-
tude of Madras, by Moon-Culminating Observations," which he
prepared at the request of the late Mr. Baily. This is an excel-
lent paper, containing some valuable formulæ and remarks. It
is printed in the twelfth volume of our *Memoirs*.

Mr. Riddle is best known by his valuable work on Navigation
and Nautical Astronomy. He had been collecting materials for
this work before he left Newcastle, but it was first published in
1824. It was an immense improvement on the empirical com-
pendiums in vogue when it appeared, combining practice and
theory in just proportions. Its leading characteristic is,—that
while it contains all the tables and rules for computation necessary
for the practical seaman, it contains also the investigations of the
rules, and the preparatory mathematical information necessary for
understanding these investigations. This is the crowning excel-
lence of the work ; and, as a proof of its admirable adaptation to
the purposes of instruction, no less than five editions of the work
have been disposed of, and the sixth, under the superintendence

of his able and talented son and worthy successor, Mr. John Riddle, will shortly be published. It is hoped that the forth-coming edition will be found to be one of the best books for nautical instruction that chn be put into the hands of seamen.

In 1822, Mr. Riddle communicated a short paper to the *Philosophical Magazine* "On the Simplification of Ivory's Solu-tion of the Double Altitude Problem." By a very simple trigo-nometrical transformation, he adapted Ivory's solution to loga-rithmic computation, and gave to it that practical working form now in use.

In 1841, the school which Mr. Riddle had so long conducted was divided, Mr. Riddle retaining the superintendence of the senior portion, which was then called the Nautical School. The mistaken zeal of some persons in the cause of education led to the appointment of a very incompetent person* to the mastership of the junior portion of the school, who, thoroughly ignorant of the heavy and much self-imposed labour in which Mr. Riddle had so long delighted, proved a source of much anxiety and an-noyance. Assertions were made to the Board of Admiralty, in reference to the progress of Mr. Riddle's pupils, which induced the Board to request two public teachers of mathematics to visit the school and report on the state of the instruction. The report made by these gentlemen completely refuted the assertions, and strongly expressed to the Lords of the Admiralty the opinion of the reporters, that the state of the school was not only highly satisfactory in itself, but, considering the low state of instruction of the pupils at entrance, and the small number of teachers allowed, a very remarkable proof of Mr. Riddle's energy and ability. It will be unnecessary further to revert to these cir-cumstances — peculiarly painful to a sensitive mind like Mr. Riddle's—than to state that they resulted, if possible, in a higher estimation of him by all who knew him, and added not a few to the list of his friends. Something of disappointment, however, remained ; neither was there from that time the same elasticity and cheerfulness which had marked his earlier career.

In 1845, a most able and efficient staff of masters had been happily gathered around him, and they united to present him with a testimonial of their esteem. On that occasion the Governor, Admiral Stopford, complimented Mr. Riddle on the firm stand he had made under some recent difficulties which had then happily passed away.

Shortly after Mr. Riddle's retirement in 1851, his bust in marble, finely sculptured by Mr. Theed, was presented to him by a large number of his former pupils and friends, accompanied with the expression of their high esteem for his worth, both as a public and private man. It was presented to him in the presence of the boys, and a large circle of friends and naval officers, includ-

* Since deceased.

ing the Governor, Admiral Sir Charles Adams, who, in a very
feeling and effective address, adverted to Mr. Riddle's long, useful,
and honourable services in the Institution.

Mr. Riddle was allowed to retire on his full salary; but he
did not live long to enjoy his cessation from active duty, as
successive attacks of paralysis issued at last in the complete pro-
stration of his physical powers. His death took place at his
residence in Greenwich, on the 31st day of March, 1854, in the
sixty-seventh year of his age.

Mr. Riddle was distinguished among the many mathematicians
who have done honour to the county of Northumberland; he was
much esteemed by all who enjoyed the privilege of his acquaint-
ance; his habits were plain, unobtrusive, and unostentatious; his
hospitality and obliging disposition will be long remembered by
those who have experienced them; and his attachment to his
friends was warm and sincere. He was partial to music; at one
time he could perform very pleasingly on the violin; and his
taste for the songs and music of Scotland, as well as the Irish
melodies and Welsh airs, led him to purchase every work of this
description on which he could lay his hands.

Mr. Riddle's success as a teacher of navigation and nautical
astronomy has never been surpassed— nor even equalled; he
brought to his task a thorough acquaintance with the minutest
details of the subject he had to expound, and his activity of
manner and energy of character carried him through almost any
amount of labour. Nor did his toils always close with the day;
at midnight, or in the early morning, he would frequently repair
to the dormitories of the boys, and rouse the elder ones, who were
accustomed to take observations of the moon or other celestial
visitant, with the quick call of "*my first class.*" Thus day and
night witnessed his unwearied exertions and unparalleled industry
in the dissemination of both theoretical and practical science among
the youth attending the Royal Hospital Schools. He was one of
the most indefatigable teachers of the age: the high status which
these schools have so long maintained, in preparing youths for sea-
service, is almost entirely attributable to his exertions and example;
and there are very many masters and commanders in the Royal
Navy, as well as officers in the Merchant Service, in every part of
the world, whose naval services are at this time testifying to the
soundness and efficiency of their early education derived from
Mr. Riddle of Greenwich. No teacher ever deserved the grati-
tude of his country more than Mr. Riddle; few have done her
more real and substantial service, and conferred so many ad-
vantages on her naval and commercial interests.

While the Council regret the loss of the services of Mr. Riddle,
who was one of their number for twenty-five years, they have the
pleasure of congratulating his son on his succeeding to the position
of Head Master of the Nautical School; and they trust that with
his high mathematical attainments and professional abilities,

and stimulated by the example of his father, his exertions in promoting the cause of nautical instruction will be equally successful and distinguished.

ST. ANDREW ST. JOHN was the second son of the Rev. Edward Beauchamp St. John, and great-grandson of Henry Lord St. John, of Bletshoe in the county of Northampton. His early education was conducted by his father until he was placed at the Plymouth New Grammar-School, under the superintendence of W. Bennett, M.A., of Trinity College, Cambridge. Here he was prepared for admission to the Royal Military Academy at Woolwich, which he entered in February 1843. During his rapid progress through the Academy he displayed mathematical abilities of the highest order. His career was most brilliant, and it is believed almost unequalled. At the final examination he far outstripped all his competitors in mathematical attainments, and obtained his commission as a Second Lieutenant in the corps of Royal Engineers.

In December 1846 he was appointed to a command at Templemore, in Ireland, and was removed from thence to Gibraltar in 1847, and in the following year to Hong Kong, returning to England in August 1851. He obtained leave till the following January, when he went to Portsmouth and Hurst Castle. On the 17th December, 1853, he sailed on the Darien Expedition, the exposure and fatigue of which injured his constitution, and he returned the following spring much impaired in health, and was appointed to a command at Devonport, but was obliged to obtain leave on a sick certificate, and was never able to return to his professional duties. His death took place at Plymouth, 21st September, 1854, at the early age of twenty-seven.

Lieutenant St. John was passionately devoted to the study of mathematics, and whenever his military duties would admit, his leisure hours were spent in the prosecution of his favourite pursuits. Not a month before his death, he told the writer of this memoir, that, if he could obtain three years' leave, his most ardent wish was to proceed to the University of Cambridge with the intention of taking a degree in mathematical honours. His published writings are to be found in vol. i. (new series) of the *Professional Papers of the Corps of Royal Engineers.* They consist of a paper on the " Equilibrium of Roofs ;" another on the " Equilibrium of the Arch ;" and a third on " Dialling." He left many valuable papers unpublished.

WILLIAM SCOTT attained his position almost wholly by his own exertions. He was born in October 1800, at Maxton, in Roxburghshire, of a good family, but which then was in reduced circumstances. He had the misfortune to lose his father at an early age; but that father, while he lived, spared from his narrow income what was necessary to give his two sons, of whom William was the eldest, the best education which a country town afforded.

At eleven years of age he was sent to the parochial school at Maxton, where he acquired a good knowledge of the English language and a fair amount of arithmetic; and three years afterwards he was sent to a school in the neighbouring town of Mertoun, in Berwickshire, in order that he might be instructed in the rudiments of classical literature, as well as extend his progress in arithmetic and algebra. Here he obtained a competent knowledge of Latin.

In November 1818 young Scott entered the University of Edinburgh, where he studied the Latin and Greek languages, besides logic, mathematics, and natural philosophy, under the distinguished professors of that time, among whom may be mentioned the names of Christison, Wallace, Leslie, and Jamieson, who honoured him with their particular friendship.

Mr. Scott began his mathematical studies under the tuition of Dr. Nicol; and the lessons of that mathematician he attended, as a private pupil, according to the practice in the Scotch Universities. By diligent application he speedily made the progress necessary to qualify himself for the mathematical class at the University, and was, in consequence, received there as a pupil of Professor, afterwards Dr. Wallace. A testimonial from Professor Wallace (1821) shows that he was one of the four students who shared the first prize for mathematics, given by the magistrates, patrons of the University. While at Edinburgh he supported himself, and paid the University dues, with what he could save from the remuneration he received for the lessons he gave to private pupils.

He was afterwards engaged at Edinburgh as an amanuensis by the late Capt. Basil Hall, R.N., whom he assisted in his astronomical pursuits; and, in preparing for the press the works of that officer, he greatly improved his style and increased his facility in English composition. In 1826, while so employed, he took his degree of M.A. in the University of Edinburgh; and, in the same year, he was recommended by his patron, Professor Wallace, on an application from the late General Butler, to fill the post of a Mathematical Master in the Royal Military College at Sandhurst.

The reputation of Professor Wallace was deservedly so high in that Institution, in which for many years he had been one of the mathematical professors, that the recommendation was immediately accepted, and Mr. Scott, being found duly qualified, at once received the appointment. He joined the Institution in February 1827. A reduction happening at that time to be made in the educational staff of the College, Mr. Scott was at first charged with the instruction of a class of gentlemen cadets in History and Latin; but, within a year, he entered upon his regular duty as a Professor of Mathematics, and this duty he continued ably to fulfil to the time of his death. The retirement of two professors, who were his seniors, placed him in a few years in the rank of first professor in the junior department of the Institution. In this situation all the energies of his mind

were devoted to his duties as an instructor, and to the composition of his lectures.

Before this time the course of study in mathematics at the College must be considered as having been very elementary; and to Mr. Scott fell, in a great degree, the task of preparing the plan, and carrying out the measures adopted, for extending the course into the higher branches of mathematical science. In furtherance of this view he published, 1844, his *Treatise of Arithmetic and Algebra*, and, in 1848, his *Treatise on Plane Trigonometry and Mensuration* — works which, without, in matter or treatment, being beyond the mental powers of the youths for whom they were intended, have the great merit of being written in a philosophical spirit ; and, had his life been prolonged, those works would have been followed by treatises on the higher departments of mathematical analysis. These he had in a great measure prepared, and in a manuscript form were in the hands of his pupils, who owe to him the knowledge they acquired of an important branch of human learning, and who cannot but remember with pleasure the general suavity of his manner, and the pains he ever took to remove the difficulties which beset the career of their professional education. Besides the works just mentioned, Mr. Scott, in 1854, published an elementary treatise on arithmetic, which is rendered particularly valuable by containing a series of elaborate tables computed for the purpose of facilitating the introduction and employment of a decimal scale of weights and measures. He was admitted a Fellow of the Royal Astronomical Society in 1835, but his distance from town, and the duties of his professorship, which required his presence almost daily at the College, rarely permitted him to attend its meetings ; he took, however, the greatest interest in its proceedings, and in the discoveries which have added of late so much lustre to the science, for the promotion of which it was formed.

In 1852 Mr. Scott was appointed examiner in mathematics of the candidates for commissions in the British army, and this delicate post he held till his death, exercising the duties with strict impartiality, and to the entire satisfaction of those in authority.

Mr. Scott's constitution had never been strong ; and in the beginning of last year (1854) his health began rapidly to decline. He died on the 8th of July ; his complaint being aggravated by unremitting attention to his studies and the duties of his professorship.

ROBERT SNOW was the eldest son of Robert Snow, the banker. He distinguished himself much at Eton and Cambridge, gaining especially Sir William Browne's medal for the best Latin ode in 1825. Since he took his degree he gave up many years to literary pursuits. Some publications, printed a few years since, prove how sincere an admirer he was of poetry, and how acutely he observed the beauties of the natural and celestial worlds. In the

work called *Memoranda of a Tour on the Continent,* he gives much information during his expedition amongst the Alps, particularly during the passage of the Col du Géant; and his journey to Sweden to observe the total eclipse, some years ago, proves his zeal in the pursuit of astronomical researches. He also published a small compendium of astronomical definitions for the use of learners.

For the last few years a painful and distressing complaint prevented his application so earnestly to his favourite studies, and at last obliged him even to give up his observations at his observatory at Ashurst.

Mr. Snow was for a long time a Fellow of the Society, and was for several years on the Council. He was at one time a regular observer, and our volumes of *Memoirs* and *Notices* contain many of his observations. He was a remarkably nice noter of phenomena, and his occultations of stars by the moon may be safely relied upon.

GEORGE BURGESS WILDIG was born at Lichfield in the year 1784. His parents were inhabitants of Betley in Staffordshire. He received his school-education at the Grammar-School of Newcastle-under-Lyne. Though remarked as intelligent and studious, he left school at an early age, and entered a mercantile house at Liverpool, in which he at length became a partner. When about twenty-five years of age he abandoned business for pursuits in which he might more easily gratify the bent of his mind towards study and meditation. With this purpose he removed to Edinburgh in 1808, and for three sessions attended classes in that university, particularly those of Professors Dugald Stewart, Christison, and Leslie. By these eminent men he was especially noticed, and was honoured with their lasting friendship. His services were given in correcting for the press some of Professor Leslie's important works. In 1811 he entered at Caius College, Cambridge, where Professor Woodhouse appreciated his merits, and used his assistance, as Leslie had done at Edinburgh. He graduated B.A. in 1815, and then went to reside again in Liverpool, entered into holy orders, and held a curacy in that town. A short time afterwards he accepted the mathematical professorship at the Liverpool Institution, which office after a few years he resigned. In 1826 he was instituted to the Rectory of Norton-on-the-Moors, near Burslem, in Staffordshire, of which parish he previously possessed the advowson. His discharge of parochial duties was marked by attention, moderation, sound judgment, and consistency. His spare time was given to his favourite pursuit of mathematical science, blended with ancient and modern literature. With the Italian writers of the best ages, and with the niceties of their language, he was intimately acquainted. The rupture of a blood-vessel compelled him in the latter years of his life to resign clerical duties, and to make his abode in a mild climate. He chose St. Heliers in Jersey, where he died on the 9th of December, 1853.

His attachment to mathematical science continued to the last, and he beguiled many of the hours of pain and weakness in the two years next before his death by an elaborate recension of every article in one of the treatises on algebra most in use in our universities. Though he has left much matter in manuscript, he rarely gave anything to the world through the press. A short geometrical treatise on the conic sections, which he printed in 1822 for the use of the Liverpool Institution, is marked by elegance, geometrical purity, and comprehensiveness. He was the author of some mathematical articles in the seventh edition of the *Encyclopædia Britannica*, the signature to which is O. O. O.; but in the list of contributors his name is erroneously given as the Rev. G. B. Wilding, M.A., *Oxford*. Many friends of similar pursuits, and many grateful pupils remain sensible of the advantages they have had from his luminous conversation and instructive guidance. His scientific strength lay on deep foundations. He was conversant with the Greek geometers, and with the discoverers, promoters, and perfectors of modern mathematical investigations from the middle ages to the present day. His remarkable talents and learning, though so little displayed to the world, were well known to many friends; among whom it is needless to mention those Cambridge men of his day, who were among the first founders, and are yet distinguished members of the Astronomical Society, and leading names in British science.

JOHN WILLIAM WHITTAKER, D.D., was born about the year 1790. He graduated at St. John's College, Cambridge, where he took his B.A. degree in 1814, his name appearing as thirteenth on the list of Wranglers for that year. He was subsequently elected a Fellow of his College; and it was while residing on his Fellowship that he published his celebrated work which introduced him to the notice of the late Archbishop of Canterbury. This was entitled " A Historical and Critical Enquiry into the Interpretation of the Hebrew Scriptures, with Remarks on Mr. Bellamy's New Translation." It was printed at the University press during 1819, and was followed shortly after by a " Supplement " containing numerous additions and corrections. In consequence of the publication of this work he was appointed Examining Chaplain for the Archbishop, who presented him to the Vicarage of Blackburn on the demise of the Rev. Thomas Durham Whitaker, LL.D., F.A.S., the well-known historian and archæologist, in 1822. He proceeded to the M.A. degree in 1817; was admitted B.D. in 1824; and completed the usual course for the degree of Doctor in Divinity during 1830. The offices of Rural Dean and Honorary Canon of Manchester Cathedral were also conferred upon him by the Bishop of that diocese.

During Dr. Whittaker's residence at Blackburn he published a series of letters on the church; and in 1825 he was appointed to preach a course of sermons before the University of Cambridge, the subject being justification by faith. This course

was published the same year, and was appropriately dedicated
to his patron the Archbishop. Besides these he also published
various pamphlets on passing subjects of controversy; one of
which was a sermon preached to the Chartists when they took
possession of the parish church in 1839; and another related
to the remains of the ancient British language in the northern
part of the kingdom. Although his parochial duties necessarily
occupied a large share of his attention, he always kept up his
scientific and general reading. Not long before his death the
writer of this notice had an opportunity of hearing him explain his
researches into the affinities of the Chinese language so far as they
had a bearing upon the cosmogony of the first chapter of Genesis;
and he listened with delight to his examination of the different
systems of computing time adopted by our leading geologists. He
had elaborated, to use his own expression, a theory of the uni-
verse, which, in his opinion, tended to remove all difficulties, and
reconciled the biblical, the astronomical, and the geological
theories. Had his health permitted, he would undoubtedly have
favoured the world with some of his researches concerning the
nebular hypothesis and geological time. He was one of the first
Fellows of the Astronomical Society, having, as the writer under-
stood, assisted in its formation. He always entertained a high
opinion of the value of the labours of the Society, but regretted
that other urgent duties had almost isolated him from subjects in
which he formerly took a deep interest.

As a preacher Dr. Whittaker never aspired to the character of
a popular orator; but the calm and argumentative style of his
sermons was calculated rather to correct the judgment of his
hearers than to rouse their feelings by appeals to the imagination.
As a writer he had few superiors in the purity of his diction and
the rhythmical composition of his sentences. His health had been
failing for some years, and he finally sank under a complication of
diseases, at the Vicarage, on August 3d, 1854, in the 64th year
of his age. The public of Blackburn honoured his remains with a
public funeral a few days after his decease, the Lord Bishop of
Manchester officiating at the ceremony.

Captain Sir JOHN FRANKLIN, of the Royal Navy, Knight Com-
mander of the Guelphic Order of Hanover, D.C.L. and F.R.S.,
was an early Fellow of this Society, and must be well remembered
by many of those who are now present. He was born at Spilsbury
in Lincolnshire, in the year 1786, entered the navy at the age of
fourteen, and was on board the Polyphemus at Lord Nelson's at-
tack on Copenhagen in 1801. Shortly after that memorable action
he joined the Investigator sloop, Captain Matthew Flinders, and
sailed on a voyage of discovery to New Holland; and it was under
that regretted officer that he imbibed that zeal for geographical
research for which he was distinguished through life.

While thus engaged, he had the misfortune to be wrecked on
a coral bank; and in returning home on board an East-Indiaman,

he was present at the celebrated repulse given to a French squadron under Admiral Linois, by Commodore Dance and his merchant-ships, on which occasion Franklin was one of those who managed the signals which so greatly deceived the enemy. On arriving in England he was immediately appointed to the Bellerophon, of 74 guns, in which well-known ship he bore a part in the great battle of Trafalgar, on the 21st of October, 1805. He served with great merit during the remainder of that arduous war, and was strongly recommended for promotion for his conduct at New Orleans.

On the general peace in 1815 his mind reverted to its former bent, and he bestirred himself in the cause of discovery. In January 1818, he assumed command of the hired brig Trent, and accompanied Captain David Buchan on a voyage of research to Spitzbergen, which led to his being intrusted with the charge of an expedition to the Coppermine River and the northern regions of America. The details of that fearful undertaking, together with his subsequent polar explorations, are too widely known to need repetition here; as is also having been some time the Lieutenant Governor of Van Dieman's Land. Suffice it to say, that he sailed in the Erebus on the 3d of March, 1845, on his last and ill-fated voyage. Since then the public feeling has been painfully excited respecting the fate of our hapless countrymen in those inhospitable wastes, and the most liberal and spirited measures were adopted, as well by our Government as that of America, and by the bereaved Lady Franklin, for their rescue; but an unusually dense mystery pervaded the whole.

The hope which glimmered faintly in the gloomy uncertainty has at length been quashed by the recovery of certain unmistakeable articles belonging to Franklin and the officers of the Erebus; and yet even this requires further evidence. The plate, the clothing, and the decorations of knighthood, which have been brought to England by Dr. Rae, form a conclusive token of a terrible catastrophe having happened to that ship; but as Dr. Rae is confessedly unacquainted with the Esquimaux language, and only gained his information through the equivocal medium of an interpreter, we must pause upon several points of the tale he has published, and especially the distressing statement that our people resorted to cannibalism. It must be borne in mind that the period of the tragedy is stated to be in 1850,—that the natives communicated with were not those who had communicated with the Franklin party,—and that there would be an obvious object in their misleading Dr. Rae. Notwithstanding, however, the accounts being unsatisfactory, there can be no reasonable doubt of the extent of the disaster. The Council have not ventured to erase the name of Sir John Franklin from our list until the Admiralty set the example: and it is with feelings of deep sorrow that they acknowledge the propriety of such a step.

Captain FRANCIS RAWDON MOIRA CROZIER was born at Bain-

bridge, in the county of Down, in Ireland, and was early destined
for sea life. He entered the navy in June 1810, on board the
Hamadryad frigate, under the command of that active officer, Sir
Thomas Staines; whom he afterwards accompanied to the Pacific
in the Briton, and made an interesting visit to Pitcairn's Island,
which they found peopled by the mutineers of the Bounty. In
1824, Mr. Crozier was appointed master's mate of the Fury,
discovery ship, under Sir Edward Parry, with whom he became
associated in three successive polar voyages; having been made a
Lieutenant in March 1826.

Mr. Crozier was afterwards employed on the coasts of Spain
and Portugal, till December 1835, when he joined the Cove, a
hired vessel, which was despatched from the Humber in search of
certain missing whalers, under the command of Sir James Ross.
Being now distinguished for science, seamanship, and fertility
of resource, his services came into demand; and in May 1839 he
was appointed to command the Terror, in which ship he accom-
panied the same officer on a voyage of research into the Antarctic
Ocean, during which time he was advanced to post rank. In
March 1845 he was re-commissioned to the Terror, and sailed on
a fresh attempt to explore the north-west passage, through Lan-
caster Sound and Behring's Strait. He has not since been heard
of!

Though the articles brought to England by Dr. Rae were
mainly illustrative of the fate of the crew of the Erebus, there
is every apprehension that Crozier and his gallant associates shared
the fate of their commander. As the seas and land on the meridian
of Cape Walker, and north of it to a distance exceeding 100 miles,
have been thoroughly searched for the missing ships without
success, it is not improbable that they are frozen up in one of the
channels between North Somerset and Banks's Land, which have
not been yet explored. It is therefore just possible that a few of
the juniors, with hardy constitutions, may survive; but the seniors
must, long ere this, have succumbed to privation and suffering.

The operations at the Royal Observatory, Greenwich, have
been characterised, during the past year, by the same activity
that has prevailed in former years. No important addition has
been made to the instruments; but those already in use have been
employed with the same inflexible steadiness and on the same
classes of objects. It was stated in the last February Report, that
the mechanism necessary for the registration of transits, by means
of electro-magnetism, was nearly complete; and we may now
add that the method has been for several months in use with
perfect success and with considerable advantage to the observ-
ations. The first transits recorded by this method were made on
March 27, 1854; and since that time all the transits, with
trifling and occasional interruptions, owing to derangements of
the machinery, have been made by the same agency. The
observations made over nine wires by this method are indisputably

of a higher order of excellence than those formerly made by the ordinary method of the eye and ear ; and great hopes are held out that the personal equations of the different observers will be comprised within very narrow limits.

The only important addition to the external galvanic communications, as they were reported last year, is that arising from the completion of the arrangements for dropping a time-signal ball at the Navy Yard at Deal. The dropping of the ball systematically came into operation at the commencement of the present year, and with perfect success as far as the general action of the machinery is concerned, though several failures have arisen through defects in the local galvanic clocks, and from other causes. All these defects have since been remedied, and there is no doubt of the facility with which the dropping of the ball can be ultimately effected as a matter of routine business ; and there cannot be a doubt of the great advantage which will accrue to this important port by the correct knowledge of Greenwich time.

The arrangements for determining the difference of longitude between the Observatories of Greenwich and Brussels were stated with some detail in the last February Report. The result was there given that was deduced from the first part of the operation, namely, a difference of longitude amounting to 17^m $29^s\cdot 256$. We may add in this place that the second part of the operations, namely, that after the interchange of the observers, gave, for the resulting difference of longitude obtained by the same mode of treatment, 17^m $28^s\cdot 538$; and that the results corresponding to a rather different mode of treatment of the star-observations, were respectively 17^m $29^s\cdot 340$ and 17^m $28^s\cdot 476$, as deduced from the two sets of operations, and that the whole number of signals on which these results are based was 1104. The definitive result of the whole series of operations is 17^m $28^s\cdot 90$, which differs by $1^s\cdot 3$ from the result previously deduced by M. Quetelet and Mr. Sheepshanks by means of chronometers. We may also remind the Society that a paper by the Astronomer Royal, which explains with full detail every part of the operations, has been printed in the current volume of our *Memoirs* recently commenced.

But the most important series of operations for the determination of longitude performed during the past year, is that by which the difference of longitudes of Paris and Greenwich has at length been definitively settled. It is well known to the Society that this determination was intended as one of the earliest applications of the galvanic method ; and, as early as the year 1851, a correspondence was commenced by the Astronomer Royal with M. Arago on the subject. Various causes contributed to the delay of the projected operations, amongst the most prominent of which were the ill-health and subsequent death of M. Arago, and the consequent unsettled condition of the administration of the Paris Observatory ; as also the failures which occurred soon after the completion of the submarine galvanic connexions with the Continent, and the delays in establishing the connexion of the Observ-

atory of Greenwich with the line of wires of the Submarine and
European Telegraph Company.

Owing to all these causes, the operations connected with the
determination of the longitude of Paris did not commence till the
month of May 1854, but they were then carried through with a
completeness and accuracy superior to all preceding attempts at
the determination of longitude.　Independently of the care be-
stowed at Greenwich on every part of the operations, the suc-
cessful issue of them was due mainly to the energetic way in
which M. Le Verrier entered into every part of the details of
his share of the work, and surmounted the successive difficulties
which lay in his way.

It has been proved, with regard to earlier attempts at deter-
mining longitudes by other methods, that, generally speaking, the
doubtful or failing point has been in the determination of local
time at the different stations.　However well rocket or other
signals may be observed, and how skilfully soever all the details
of the operations may be conducted, yet any errors affecting the
time as obtained by the transit instrument, whether from faulty
construction, bad determinations of instrumental errors, or un-
eliminated personal equations of the observers, are fatal to the
whole operation.

On entering upon the Directorship of the Paris Observatory,
M. Le Verrier found his transit instrument very defective, and
utterly unfit, without great alterations, for the delicate work
required of it.　With his characteristic energy he proceeded im-
mediately to remedy this serious defect ; and, though he was
obliged to put up with considerable delay, he at length got the
transit instrument reinstated in a perfectly satisfactory condition.
To this cause, as well to his minute attention to the instrumental
errors during the whole course of the operations, is mainly
due the confidence which we repose in the final determination.

With regard to the operations themselves, they consisted, as in
former instances, of two series ; in the first of which M. Faye, on
the part of the Paris Observatory, observed stars and galvanic
signals at Greenwich, while Mr. Dunkin, on the part of the
Greenwich Observatory, made the similar observations at Paris.
In the second series, the observers were reversed — M. Faye
observing at Paris, and Mr. Dunkin at Greenwich.

In the first series, the observations commenced on May 27, and
terminated on June 4, 1854; five evenings during that interval
being made available for the determination of longitude by the
observation of transits of stars as well as galvanic signals at
both observatories.　In the second series, the observations com-
menced on June 12 and terminated on June 24, seven evenings
being available for longitude.　The determination from the first
series, as derived from more than 700 signals, was that the east
longitude of Paris is 9^m $20^s \cdot 50$; and that from the second series,
as derived from nearly 1000 signals, gave for the east longitude,
9^m $20^s \cdot 76$.　The definitive result is, therefore, 9^m $20^s \cdot 63$, differing

by nearly one second of time from that formerly determined by means of rocket signals.

In the course of the last summer, an important series of experiments was instituted by the Astronomer Royal for determining the variation of gravity on descending to a considerable depth in a mine, with the ultimate view of inferring from the result of this observation the mass, or the mean density, of the earth. Without referring to the indirect determinations of the law of density from the figure of the earth and the motions of the moon, the direct determinations hitherto made use of are of two classes,—one the attraction of Schehallien; the other the attraction of leaden balls, as observed by Cavendish, Reich, and Baily. In both these methods there are peculiar difficulties, and it appeared, therefore, at least desirable that a trial of a different method should be made. And it appears on calculation, that the probable effect of unavoidable errors when the mean density is inferred from experiments in a mine is less than in the other methods. These considerations induced the Astronomer Royal many years ago, in conjunction with Dr. Whewell, Mr. Sheepshanks, and other friends, to institute pendulum experiments in the Dolcoath mine in Cornwall. In two different years the experiments were tried, and in both they failed in consequence of local accidents. In the experience, however, of these abortive attempts, the observers learned that the principal inaccuracies arose from the difficulty of comparing a clock at the surface of the ground with one at the bottom of the mine. In the year just past, the observers at Greenwich acquired considerable practical familiarity with the manipulations of the galvanic telegraph; and it became evident to the Astronomer Royal that by its agency the difficulty, so formidable in other years, would now be easily overcome. Accordingly, he proceeded in the summer to inquire into the circumstances of different mines in the great Durham coal-field, and he found that the Harton Colliery, near South Shields, reported to be 1260 feet deep, was admirably adapted to the experiments, and that the owners of the mine were anxious to give every assistance. An expedition on a competent scale was soon prepared. It was necessary to obtain the assistance of observers from several English observatories. The working party, as finally arranged, consisted of Messrs. Dunkin and Ellis from the Royal Observatory, Mr. Rümker of the Durham Observatory, Mr. Pogson from Oxford, Mr. Criswick from Cambridge, and Mr. Simmonds from the Red Hill Observatory. Pendulums were lent by the Royal Society. The labour and skill of telegraph engineers, required in establishing simultaneous galvanic signals between the upper and lower stations, was gratuitously supplied by the liberality of the Electric Telegraph Company. The Admiralty assisted by a grant of a sum of money. The observations consisted of 104 hours of incessant observations of one pendulum (A) above and another (B) below; then of 104

hours with (B) above and (A) below; then of 60 hours with (A) above and (B) below; then of 60 hours with (B) above and (A) below. The result, as to the mechanical firmness of the instruments and trustworthy character of the observations, is most satisfactory. The first conclusion is, that the lower pendulum is accelerated $2^s \cdot 25$ per day, or that gravity is increased at the lower station by $\frac{1}{19100}$ part. Operations are now in hand for the precise measure of the depth of the mine, for ascertaining the general form of the country, and for obtaining the specific gravity of the rocks; and till these are completed the final result for the earth's mean density cannot be obtained. It appears, however, likely to prove high,—between six and seven times the density of water.

The publication of the thirteenth volume of the *Radcliffe Observations* has been already mentioned in the *Monthly Notices*. It contains the continuation of the observations for the circumpolar catalogue to the end of 1852. The *Observations* with the *Heliometer*, the publication of which was unavoidably delayed, are now printed, and the results are in the hands of the Society. They will appear, with the rest of the fourteenth volume, in the course of a few weeks.

It was mentioned in the last Annual Report, that they had been principally directed to the determination of the parallax of 61 *Cygni* and 1830 *Groombridge*. The partial result there mentioned, which gave a parallax of $0'' \cdot 384$ to 61 *Cygni*, has been fully confirmed. Subsequent investigation, taking in a greater number of observations, having only altered that value to $0'' \cdot 392$ or $0'' \cdot 402$ (accordingly as a temperature correction is used or not), with a probable error of $\pm 0'' \cdot 015$.

The observations of 1830 *Groombridge* have presented greater difficulties, and apparently an anomalous result, inasmuch as they assign a greater parallax to one of the stars of comparison than to 1830 *Groombridge*. Dr. Wichman, it will be remembered, fell upon a similar difficulty; and what adds to the perplexity is, the star which appears to have the greater parallax in this case is one of those which in Dr. Wichman's researches appeared to be most distant.

This is not the place to enter into a disquisition on the subject, but a short statement of the circumstances may not be uninteresting to the Society.

The stars selected for comparison with 1830 *Groombridge* are the same which Otto Struve used, but they have been designated in contrary order, the star which he called *b* being the *a* of the present inquiry. The same stars were also observed by Dr. Wichman in his more recent researches, in connexion with a *third* star. The two stars now in question are Wichman's *a* and *a''*. The comparative measures between 1830 *Groombridge* and the former give a parallax of $0'' \cdot 26$ to 1830 *Groombridge*; whereas in the other case the resulting parallax is $-0'' \cdot 18$, or, in other

words, the parallax of Wichman's *a″* (the *b* of the present inquiry) is 0″·44.

Dr. Wichman found the parallax of 1830 *Groombridge* 0″·72, and that of his *a* (which is also *a* in this case) 1″·17 (*Astron. Nach.* No. 844, p. 50).

It were vain, in the present state of our knowledge of the structure of the heavens, to attempt to argue on the abstract probability of results such as these. It will appear to many minds just as improbable that a star having a proper motion of 7″ of the great circle *should* be immeasurably distant from us (for this is the alternative), as that another having no physical peculiarity *should not* be.

It is not on such argument that Mr. Johnson founds his suspicion of the accuracy of the result at which he has arrived, but on the simple fact of two observers, with instruments similar in principle, though somewhat different in construction, having arrived at such opposite conclusions.

In his opinion, this seems to show some imperfection in double-image measures of objects separated by large arcs, which still remains to be explained.

It is true Dr. Peters has shown most ingeniously (*Astronomische Nachrichten*, No. 866) that Wichman's adopted temperature correction is probably too small. But he has not shown that the amended correction will reconcile independent results, so that each star separately will tell the same story; and until this has been done, Mr. Johnson conceives that his objection is not refuted.

The strongest argument in favour of Mr. Johnson's result, as it stands, is, that the observations of 61 *Cygni*, where the comparison stars are more unfavourably situated with regard to distance, show no such anomaly; and both series were carried on, in great part, contemporaneously. The parallax of 61 *Cygni*, with regard to its stars of comparison, is a maximum in April and October, about the times of the mean temperature of the year; while the maximum parallax of 1830 *Groombridge* occurs in December and June, at the extremes of annual temperature. Any disturbance, therefore, produced by expansion might affect the latter determination without affecting the former. But it happens that, of the *four* independent cases which have come under discussion, *one* only has shown symptoms of such disturbance, amounting to −0″·11 for an increase of 10° of Fahrenheit on a distance of nearly 2100″; while in *every* case the accordance of the separate results is improved by rejecting a temperature correction altogether. There was, however, this difference in the optical condition of the instrument in the two cases. In the case of 1830 *Groombridge*, in order to render the small stars visible when in juxtaposition to it, the aperture of one segment was reduced to about two inches, while with 61 *Cygni* no such reduction was necessary. Mr. Johnson is at a loss to explain *how* this circumstance can affect the accuracy of com-

parative measures: it is mentioned as being the only apparent source of error which was not common to both series.

The observations for the Circumpolar Catalogue have at length been brought to a close. Some reasons for the delay which has occurred have been before mentioned. Besides these, another was the necessity of investigating into the cause of a small difference which has often been noticed between the N.P.D. given by the Radcliffe Observations and by other authorities.

Mr. Johnson has now satisfied himself on this point. A small excess of the adopted over the true latitude, amounting to about $+0''\cdot35$, and an error in the adopted constant of refraction (Bessel's) of about $+0''\cdot2$, will explain a portion of the difference. But the principal cause is the discrepancy between the horizontal points in different parts of the instrument, as given by direct and reflexion observations. Here we have another confirmation of the propriety of the warning often given to observers by the Astronomer Royal, not to trust to any single point for the determination of this important element of reduction. The effect of the accumulated errors will be to increase the N.P.D. about $1''\cdot25$.

Considerable progress has been made during the last six months in bringing into action the photo-meteorographic instruments which had been some time erected. They consist of a barograph and thermograph, designed by and constructed under the superintendence of Mr. Ronalds, late of the Kew Observatory, to whose disinterested zeal and attention Mr. Johnson wishes to make this public acknowledgment.

The photographic process remained to be arranged, and for this purpose Mr. Johnson has been fortunate in securing the services of Mr. Crookes, a gentleman well known for his skill, and for several improvements he has introduced into the art. Under his direction everything has been very satisfactorily completed, and the observations are now being regularly carried on.

While on the subject of the Radcliffe Observatory, though not forming part of its strictly official routine, it would be unjust to pass unnoticed the personal labours of Mr. Pogson at intervals when released from his immediate duties.

Observations of the smaller planets at important parts of their orbit with the 10-foot equatoreal and ring-micrometer,—the determination of the periods of several known variable stars, and search for new ones,—the construction of a set of maps to connect Mr. Bishop's with those of the Berlin Academy, are subjects which engage his attention as circumstances allow. His progress cannot, of course, be that of a person exclusively devoted to this kind of research. But by system and perseverance he has accomplished a great deal, and it is hoped that no long time will elapse before the Society is put in possession of some of the fruits of his exertions.

Mr. Pogson was one of the observers whom the Astronomer Royal selected to take part in his important experiments at South

Shields. These experiments afford, as far as we are aware, the first example of the mutual assistance one observatory may render to another,—an example which, we are sure, will be hailed with satisfaction by every astronomer. Apart from the direct advantages which accrue to men of kindred minds from the interchange of ideas, such intercourse between separate establishments serves to extend their sympathies beyond the sphere of local operations, and to remind them, while labouring each in his special vocation, that they are also members of an astronomical commonwealth, in the progress of which they have a direct concern.

At Cambridge, Professor Challis is proceeding steadily with the usual work. He has recently gone through a series of observations and calculations for determining the effect of the forms of the transit-pivots on the calculated times of meridian transit. Two sets of measures were taken, on different days, with the micrometer microscopes provided for bisecting dots at the ends of the pivots, the transit-axis being in one position, and two sets were similarly taken after reversing the instrument. The result of the calculations exhibited an effect of the same kind as that found by the trials made in 1850, but greater in degree. By the experiments of 1850 and 1854 the effect of the forms of the pivots on all the transit observations from the year 1850 inclusive, to the present time, may be eliminated. The more immediate object of the experiment of 1854 was to correct for the forms of the pivots, the determination of the longitude made last year by galvanic signals. Professor.Challis was unwilling to publish a final result till this source of error had been got rid of. The calculations for this purpose, which were of great length, are now completed; and the final determination of the longitude by this method is 22$^{s\cdot}$70 east of Greenwich, which is about eight-tenths of a second less eastward than the longitude hitherto used. Professor Challis considers this result to be trustworthy, and proposes to adopt it in future. A detailed account of the galvanic experiment, and of the calculations connected with it, has been submitted by him to the Cambridge Philosophical Society.

Last summer he was engaged upon a plan for mounting collimators for the purpose of determining the effect of flexure in observations with the mural circle. As this instrument is of large dimensions, being 8 feet in diameter, the effect of the alteration of form caused by the mere weight of the parts is of sensible amount. He has hitherto attempted to eliminate this source of error by the comparison of direct and reflexion observations of the same star. This method, though generally good, fails, or becomes uncertain, when the stars are less than 25° above the horizon. On this account he proposes to use collimators so mounted as to be capable of collimating with the circle telescope in any position of the latter, being carried about the circle by opposite arms which revolve round a horizontal axis. The stage for mounting the collimators is prepared, and the collimators have been received from

Mr. Simms, so that this method will soon have a trial. If successful, it will supersede the necessity of reflexion observations, which occupy a great deal of time, and are troublesome on account of failures arising from disturbances of the mercury. In any case, however, reflexion observations will be taken of stars near the zenith, in order that the determination of zenith point may not depend on a single set of divisions of the circle, which would happen if the zenith point were determined exclusively by the collimating eye-piece.

Steady progress has been made in the reduction of the meridian observations of stars near the elliptic, contained for the most part in the *Histoire Céleste,* and in Weisse's Catalogue reduced from Bessel's Zones. A considerable number of errors has been detected in these two catalogues, and some also in the catalogue of the British Association.

Mr. Carrington has during the past year been steadily pursuing the plan of observation proposed for his Observatory at Red Hill which was put forth in the *Monthly Notices,* vol. xiv., No. 1. Of the region comprised in the maps which he presented to the Society, the portion within 4° of the north pole has been exclusively under observation during 1854; and, with the exception of a mere trifle, is now exhausted. It was found that about one-sixth of the stars laid down in the maps were too faint to bear the slight amount of illumination necessary for the wires to be seen with any sharpness, and these have, consequently, been struck off the observing list. The remainder, which is believed to comprise all the stars not below the 10½ magnitude, numbers 709 stars, of each of which, with very few exceptions, four observations, at least, have been procured in both elements. There are nineteen stars observed within 40' of the pole, of which the nearest, situated in about 16ʰ by 0° 5', may be accepted as an example of his lowest magnitude, the 10½.

The companion to *Polaris* is rated at rather higher than the tenth. The reductions are also steadily progressing; but, from the additional labour required in the neighbourhood of the pole, are not likely to be concluded before Midsummer next. His transit-circle has given him the greatest satisfaction; but although he has evidence enough before him to show that its permanence of adjustment is nearly unrivalled, he thinks it better to defer any statement on the point till his reductions are more advanced. In the ensuing year, the zone 4° to 7° will be under observation, and, it is hoped, will likewise be exhausted within the year.

Throughout the past year the solar spots have been observed in position and sketched on every available opportunity. The method of observation is that which was explained in vol. xiv., No. 5, of the *Notices;* but the form of reduction has been modified in a manner which will shortly be published. It is sufficient now to state that the sun has been thus viewed during 1854 on 153 days, on thirty of which no spots were visible; and that the

number of observations of nuclei and detached spots is 328, the whole of which are finally reduced and diagrammed, showing the appearance on the disk, and the heliographical longitude and latitude, the latter quantities computed to the nearest minute of arc. Mr. Carrington has not hitherto found time for their special discussion to any extent beyond this point. He intends to continue their observation and reduction on the same system during the present year. Mr. Carrington has likewise, from time to time, during the year, been engaged in the revision of the observations taken by him at Durham; and as the printer has some time since received the whole of the copy for the press, and about half the sheets are worked off, the Society may shortly expect to receive the second volume of the Durham Observations, which will be about the size of the first one.

The principal work of the Edinburgh Observatory during the past year has been the making, computing, and printing of the ordinary meridian observations of stars.

The several steps alluded to in our last Report for enabling the Observatory to undertake a larger amount, and a more special department of business, though advanced a stage further with the authorities, are not yet confirmed and allowed. But during the past year particular questions have been examined into as follows: the details will appear elsewhere.

1. The condensation of the luminiferous ether in the neighbourhood of the sun. The probability of such a condensation having been pointed out last spring by Prof. W. Thomson as resulting from the dynamical theory of heat, Prof. Piazzi Smyth proceeded to institute observations to test the fact, and measure its amount. The operation proved very tedious on account of atmospherical difficulties preventing the visibility of stars in the immediate neighbourhood of the sun; and, in fact, only two observations have been procured of a class fit to be employed in so delicate an inquiry. That these two observations unite in showing the fact of such a condensation, and give for the amount of it at $12°$ distance from the sun; one of them, ·03 sec., and the other, ·04 sec., is, Prof. Piazzi Smyth thinks, the result of accident; but he also thinks that we may safely conclude from them that while the quantity is certainly small, it may be sensible to a long series of good observations.

2. The physical character of the surface of the Mare Crisium in the moon, at the request of the British Association.

3. The application of electric agency to improve the accuracy of time observations.

4. The improvement of astronomical observations at sea, under circumstances to which the principle of Hadley's quadrant does not apply.

At the Liverpool Observatory, Mr. Hartnup's attention has of late been directed towards obtaining photographical pictures

of the moon. At our June meeting, several specimens were exhibited to the Society, which were exceedingly promising, notwithstanding the imperfection of the apparatus for displaying them. At the meeting of the British Association at Liverpool, photographs of the moon were shown in St. George's Hall, which are said " to have outstripped all other attempts made elsewhere," and " to have superseded all maps of the moon now in existence." Mr. Hartnup does not think that, *at present*, his pictures quite deserve such praise; but he feels great hopes that they will do so before long.

The sensible defect is apparently in the *chemical* part of the process, for he finds that when the picture is magnified from 1·3 inches to 50 feet diameter, there is no fault which can be traced to the *motion* of the telescope. He conceives, too, that he is not yet " master of the best method of copying and enlarging the original pictures." It may confidently be expected that an experimenter of Mr. Hartnup's zeal and capacity, aided as he is, and will be, by the best chemical talent, will bring this art speedily to perfection; and that we shall soon have portraits of the moon painted by herself, and in which the drawing and effect are perfectly true. The last and most minute details must always probably be *directly* observed; but even an incompetent artist can fill up such details, when he has a good photographical sketch on a large scale before him. If any change is going on in our satellite, it may thus perhaps be made sensible in time.

At the same meeting of the British Association, the meteorological results of the Liverpool Observatory obtained the most flattering approbation. The Association has good reason to be proud of its creation, for the Liverpool Observatory sprung from the recommendation of that body in 1837, and few projects have been more successful. The anemometer results, diagrams, &c., were ordered to be printed entire in the next volume of the Proceedings of the Association.

The special object, however, of the Liverpool Observatory is that which was also the original object of the Royal Observatory,— to render navigation more safe. This humane and patriotic purpose has always been *first* in Mr. Hartnup's mind; and he has been furnished with the most ample means by his patrons. We have repeatedly attempted to draw attention to this most important subject, and Mr. Hartnup himself has taken every opportunity to press the matter on those most interested, but hitherto with only very partial effect. In chronometers of the usual construction, it is well known that the *compensation* for *heat* and *cold* is *imperfect*, but that the errors in a well-made time-keeper follow a certain fixed course, and can be tabulated for each instrument according to the indications of the thermometer. It has been proved by actual experience, that out of 100 chronometers, 95 will perform satisfactorily, where this special correction is applied. Now Mr. Hartnup supplies a table to every chronometer rated at the Liverpool Observatory, assigning the rate according to the tempe-

rature in which the watch is going. The additional trouble is merely this. A thermometer of the rudest kind must lie near the time-keeper, and be noted once a-day, or, when the temperature is steady, once a-week. In bringing up the error, the seaman must use the daily rate *corresponding to the thermometer*, instead of *one* rate for the whole voyage. This operation would scarcely cost an additional minute a-week; and it would make every sound, well-made, but imperfectly compensated, chronometer, as good as a perfectly compensated chronometer, if such a thing ever exists. This method, however, is practised by but a small number of the English captains who sail from Liverpool, although they would have nothing additional to pay for the advantage of being rated in this manner at the Observatory. This fact, if it were not un-deniably true, would surely seem incredible, that men should risk their own lives, those of their crew, and of their passengers, rather than take the trouble to use a rate corresponding to the daily temperature, instead of one rate for the whole voyage. Let us hope that the interest which has lately arisen with respect to the merchant service may be directed to this point.

With the consent of his Committee, Mr. Hartnup has been ap-pointed an agent of the Board of Trade for the issue of instru-ments, logs, books, &c., at the Port of Liverpool. In matters of vital importance we cannot regret the appointment of so trust-worthy a person, though astronomy may somewhat suffer. It must, however, be clear that in directing the attention of the Li-verpool astronomer to so many objects, some of them must be neglected, unless due provision be made and assistance given. Sometimes, through thoughtlessness, more work is imposed on a willing man than human force can sustain; and we may venture to hint to the munificent patrons of the Liverpool Observatory that the limit, in Mr. Hartnup's case, has already been attained, and cannot be exceeded safely. Yet it would be a great pity to stop, or even to reduce, permanently, the astronomical work of this establishment, after so brilliant a career. The Observatory of Liverpool cannot well be spared, now that it has shown how useful it is and can be.

The Madras Observatory has put forth another volume during the past year, containing the results of observations from 1848 to 1852, while an Appendix brings up a certain portion of them to the beginning of 1854.

The most important part of the contents is a " Subsidiary Ca-talogue of 1440 Stars," being the result of a thorough revision of the British Association Catalogue between the limits of N.P.D. 40° and 155°, with notes, giving particulars of proper motion and other interesting points. This was a piece of heavy work re-quiring pretty constant labour for about four years, in observing and reducing. The agreement of the places with those given in the British Association Catalogue is, in the great majority of cases, pretty close, but there are about 100 whose errors are

large, exceeding 1ˢ of time or 10″ of arc; some of these are mere blunders of whole minutes of time or arc, and a few may be cases of proper motion. Fifty-five numbers were missing, and the objects set down in the Catalogue as nebulæ appear, as far as they have been examined, to be loose clusters of small stars.

In the Planetary Observations, *Neptune* for the first time makes his appearance, and has been observed pretty assiduously since 1849; a few attempts were also made on some of the new planetoids, but they were speedily abandoned for want of sufficient optical power.

There is a pretty long list of double stars (or rather two lists, there being a second in the appendix), in which *α Centauri* holds a conspicuous place, having been carefully watched, and showing an angular motion of 30° in less than four years. These observations were made with the new equatoreal, by Lerebours and Secretan, of $6^{in.}·2$ aperture and $89^{in.}$ focus, which seems an effective instrument, though the object-glass originally furnished was of bad quality; but this defect has been remedied by the makers supplying a new one, which appears unexceptionable.

The observations in the appendix were all taken with this new glass; and we notice some pretty careful measures of *Saturn* and his rings, including the new features of the obscure ring, and fine dark line in the outer ring. There is also a second list of double stars, in which the most notable points are the indications of the binary nature of *α Piscium*, and of the parallax of *α Herculis*, which last is thoroughly discussed at the end, and shown to be = 0″·060 ± 0″·0041, from the measures of position, a result nearly confirmed by those of distance; so that the quantity, small as it is, must be considered as *real*, until it can be disproved by observations equally numerous and accordant.

In April 1854, Captain Jacob was compelled to quit his post in search of health, and is now in England, his *locum tenens* being Major Worster of the Madras Artillery. The work now in progress there is an examination of those stars, especially Southern stars, affected (or supposed to be so) by proper motion to the extent of 0″·5 annually; these amount to about 400.

The present state of the Observatory, as regards *matériel*, would seem susceptible of some improvement. The meridional instruments are rather behind the age. Originally small, and with some defects of construction, they have not been improved by somewhat rough usage for about a quarter of a century. The mural circle has large errors of division, by which most of Taylor's early observations are vitiated; and though these are now tabulated, and applied to every observation, this of course involves loss of time in reduction, besides being an eyesore. The transit instrument was seriously injured some years back in a hurricane, and though pretty well repaired is still not perfect; its optical power is too small to allow of its being used effectively on the new planetoids, a class of observation for which the geographical position of Madras is well suited.

It is to be hoped that ere long the liberality of the Court of Directors will furnish this Observatory with a meridian circle similar, if not equal in size, to those lately erected at Greenwich and the Cape. Such an instrument would have been invaluable in the revision of the B.A. Catalogue above referred to; and there can be no doubt that it would materially add to the efficiency and usefulness of the Observatory.

At the last meeting of the Committee for restoring the standards of weight and length, the members were of opinion that a larger number of standards would be required than had been originally contemplated. Mr. Sheepshanks therefore applied to Mr. Simms for *all* the bars in his stock, which was readily complied with, Mr. Simms reserving only one for his own use. These additional bars have now been most carefully prepared, divided, and measured, so that the number of standard bronze yards amounts to forty. There are four more, which were cut down to 37 inches by Mr. Baily; but he left them undivided. These have been divided and compared. Altogether there are forty-four bronze yards and several more in other metals.

There is, and always must be, some uncertainty about the bisection of anything so imperfect as the best division is when highly magnified; and this uncertainty cannot be eliminated, as it varies, *accidentally,* with each division and each observer. But with the *same* observer a very satisfactory result is soon arrived at. When the divisions are fairly defined, 100 comparisons have a probable error varying from about a seven-millionth to a twelve-millionth of the whole, or somewhere about four millionths of an inch. This *degree* and *kind* of *certainty* has been arrived at in all cases. The bars, which, being observed in different manners, presented any anomaly, have been all re-measured.

Notwithstanding the elegance and security of Professor Miller's system (that of cutting the division at the bottom of a cylindrical well, which goes half-way through the bar), Mr. Sheepshanks is of opinion that it has considerable disadvantages. Even in Mr. W. Simms' hands, and with especial contrivances, it is rather uncommon to find both the gold pins, and both the divisions, perfectly satisfactory. The small aperture does not allow the surface to be worked to a uniform degree of *dead* brightness; the burr cannot always be well cleaned off without polishing or rounding the edges; and when in use, it is not easy to wipe off any occasional dirt which may interfere with the divisions. For bars which are to be floated in mercury the *well* is essential, but for ordinary purposes, Kater's plan of cutting away half the upper substance of the bar would be found more convenient. If, as Mr. Sheepshanks suspects, the *accuracy* of the measure depends *mainly* upon the sharpness and cleanness of the division, and the perfect uniformity of the face of the dot, every facility should be given to the workman to get at his work.

Besides the anomaly heretofore mentioned, viz. that practised

and steady observers differ uniformly as to what *is* a bisection, Mr. Sheepshanks finds that the bisection greatly differs in his own case, according as the division crosses the eye vertically or obliquely. This difference is very large, with him, between the vertical and horizontal direction. Mr. W. Simms says that *he* does not find this to be the case, but he has heard it remarked by others. It will be odd enough if there is a sensible personal equation in *position* and *distance;* and it would be worth each observer's while to make out whether it is so or not, in his own case.

In making *end-yards* with *steel* tips no difficulty was found; but by an oversight, which Mr. Sheepshanks can scarcely explain, or excuse himself for, the bars hitherto made, tentatively, have all been too short. In other respects they were satisfactory. But even the comparisons that were made, though few in number, showed an evident *wearing* in the metal, and it seemed hopeless to *preserve* a standard touch-yard in use (though it is easy to get one) unless some harder material could be applied, and, if possible, one not corrosible. Professor Miller recommended quartz, and after one or two trials it was found very practicable to insert a conically-shaped piece of quartz into an iron or bronze socket, moderately heated. But when this was exposed to the cold of a freezing mixture, the quartz crashed: it was held firmly enough.

It was then considered that perhaps an intervening bed of soft metal would make the squeeze of the enveloping bronze or iron more uniform and more endurable; and Mr. James Simms further suggested that chalcedony would be found tougher than quartz. A request was made to Messrs. Elkington to deposit a layer of gold in a bed for the stone; but as this was declined by the firm in London, Mr. James Simms had the kindness to attempt to supply the want. Before, however, his attempts were perfectly successful, it was suggested that the chalcedony plug, which is very slightly conical, might be inserted in a heated gold or silver socket; that this again might be worked into a conical form and similarly inserted, in a reversed position, in the bar intended for a standard. A piece of chalcedony was thus fixed in a brass cylinder, which was afterwards turned very nicely and let into an iron socket, exactly representing the terminating portion of the end-bar. Exposure to a freezing mixture had no effect upon this combination, and the chalcedony was found to be of great toughness, such as would bear a considerable blow without injury. Some standards of end-measure would have been made before now, but the engineers have been so busy, that an order for steel bars has only just been executed. If any one can suggest a tougher or handsomer stone than chalcedony, and one which can be easily procured and of some size, Mr. Sheepshanks would feel greatly obliged by the information; but, so far as is known at present, this stone will do very well.

Besides comparisons of the discrepant bars and of the additional bars, the brass tubular scales which were measured in 1836 by Mr.

Baily, have been compared with the new standard. The Royal Astronomical Society's standard, contrary to what was expected, was found to be nearly unaltered, that is to say, supposing the new standard to be exactly equal to that which is lost. But the other tubular scales presented considerable differences, viz., the Danish standard and those of Mr. Baily and Mr. Simms. All have been carefully determined, with a probable error considerably less than the hundred-thousandth of an inch.

The question now arises, how are these discordances in the tubular scales to be accounted for? And to say the truth it is not easy. Some further attempts have been made to ascertain the effect of the strain which undoubtedly exists in the apparatus used by Mr. Baily; but the results were not consistent. It seemed that the effect of the strain upon the microscopes was influenced by the greater or less friction which attends the motion of the frame, and which depends partly on the temperature. There are two evident objections to the tubular scales: they are too long, and they are very inadequately supported, at only two points. Mr. Baily's measuring apparatus is undergoing some alterations and improvements with a view to the subdivision of the yard, and the comparison of measures other than yards.

A yard with its Miller's stand has been prepared for the French Government, and will be conveyed to France in time to make its appearance at the Exposition.

The commission of the Government which, on Mr. Baily's death, was undertaken by Mr. Sheepshanks under the direction of the Astronomer Royal, is now brought to a conclusion, though there are several small matters to attend to which will take some time. But though we may hope that there never can be any doubt in future of the *length* of the *yard*, a great deal remains to be done to make the result of this long and expensive affair practically useful. For the business of ordinary life, standard-end bars and beds are required, and types, probably, of the units most frequently in use. It is very desirable, too, that accurate comparisons should be made between the English yard and the measures of those countries which possess an accurate standard. A well-divided scale of 40 inches is absolutely required for such researches and comparisons; and it would seem a necessary completion of the work, to connect the archetypes of the great national surveys with the new parliamentary standard yard. Whether any considerable portion of that which has just been described, can be effected by Mr. Sheepshanks is doubtful. The work hitherto has pressed rather heavily on his eyesight, if not on his health, and might now, probably, be advantageously transferred to younger and abler hands. But if the Government do not object to supply the necessary apparatus, and if he can satisfy himself that the work is competently executed, Mr. Sheepshanks is willing to continue his operations some steps further. He relies mainly on the advice and counsel of the Astronomer Royal, and on the good-will,

intelligence, and beautiful workmanship of Mr. Simms and his house.

Within the past twelvemonth our knowledge of the group of small planets between *Mars* and *Jupiter* has been increased by the addition of six new members, which have been respectively named *Bellona, Amphitrite, Urania, Euphrosyne, Pomona,* and *Polyhymnia.*

Bellona was discovered by our Associate, Dr. Luther, director of the observatory at Bilk, near Dusseldorf, on the night of March 1. The period of revolution is 1700 days, or $4^{\text{yrs}}\cdot655$. The name was selected by Professor Encke.

On the same night, but about two hours later, Mr. Marth detected the planet which, on Mr. Bishop's suggestion, has been called *Amphitrite,* at the observatory of South Villa, Regent's Park. On the following night it was independently found by Mr. Pogson, who has been for several years attached to the Observatory of Oxford, where he devotes his leisure hours, after the regular duties of his office are completed, to the formation of charts of small stars, with the view to the detection of new planets or variable stars. A third independent discovery was made at the Imperial Observatory of Paris on the 3d of March by M. Chacornac, who is engaged on similar researches. The period of *Amphitrite* is 1484 days, or $4^{\text{yrs}}\cdot063$. The eccentricity of the orbit is less than that of *Ceres,* being the smallest in the group.

The *third* planet of 1854, called *Urania* on the proposition of Professor De Morgan, was found by Mr. Hind at Mr. Bishop's observatory on the night of July 22d. The time of revolution is about 1332 days, or $3^{\text{yrs}}\cdot647$, but has not yet been very satisfactorily determined.

Euphrosyne was found in a rather singular position on the 1st of September by Mr. Ferguson, of the Observatory, Washington, U.S. It was so close to the planet *Egeria,* of which Mr. Ferguson was in search, that it appears to have been observed along with it on that evening,—possibly from uncertainty as to which was the right object. Another night's observation proved that both were planets, the new one appearing of about the same degree of brightness as *Egeria.* The period of revolution is, probably, the same within a few days as that of *Hygeia* or *Themis.*

Pomona, so named by M. Le Verrier (who, as the Society will be aware, has succeeded to the direction of the observatory at Paris), was discovered by Mr. Hermann Goldschmidt, an amateur astronomer resident in that city, on the 26th of October. We already owed to the same gentleman the discovery of *Lutetia* in 1852. The approximate period is 1518 days.

Polyhymnia, the last of the six planets of 1854, also named by M. Le Verrier, was detected at the Paris Observatory by M. Chacornac on the night of the 28th of October. The elements are remarkable for the very large eccentricity they exhibit, whereby

the difference between the perihelion and aphelion distances amounts to a diameter of the earth's orbit. The time of revolution appears to be about 1787 days, or $4^{m}·892$.

Since the last anniversary four new comets have made their appearance.

The *first* of these was for some time a very conspicuous object in the western sky. It was first seen in the south of France on the morning of March 24, the day before its perihelion passage. In this country it was remarked by several persons after sunset on March 28, and became generally visible on the following evening. It decreased rapidly in brightness, and disappeared after an interval of three weeks. A parabola satisfies the whole series of observations.

The *second* comet was discovered by Mr. Klinkerfues at Göttingen on the night of June 4, and three weeks later by Mr. Van Arsdale at Newark, in the United States. At the time of its perihelion passage it became visible to the naked eye. A sensible ellipticity was at first suspected, but it has not been confirmed. The whole series of observations, which extends to the end of July, cannot well be reconciled with a time of revolution of less than several thousand years.

The *third* comet had not less than six discoverers. It was found on September 11 by Mr. Klinkerfues at Göttingen; on the 12th, by Mr. Bruhns at Berlin; on the 13th, by Mr. Van Arsdale at Newark, N.J.; on the 18th, by Dr. Donati at Florence, and Miss Mitchell at Nantucket, U.S.; and, lastly, on September 21, by Mr. Gussew at Wilna. The observations, so far as they have been published, extend to the middle of November. The orbit does not appear to differ sensibly from a parabola.

The *fourth* comet, which is still visible, was found at the same hour, on the morning of January 15, by Mr. Winnecke at Berlin and M. Dien at Paris. It is faint, and very unfavourably situate for observations, so that they are not likely to be long continued. The perihelion has been some time passed. •

The application of photography to astronomy is making sensible progress. Of Mr. Hartnup's proceedings we have already spoken. Mr. Phillips, at Oxford, has been engaged since the summer of 1853, and has furnished an account of his proceedings, which, it may be hoped, he will enlarge for insertion in the *Monthly Notices.* With an achromatic of $6\frac{1}{2}$-inch aperture, and 11-foot focal length, he first computed the *probable* place of the photographic focus, ·75 to ·80 of an inch beyond the focus. Trial proved that the photographic effect was evanescent within the focus, feeble at the focus, and greatest at about ·75 of an inch beyond the focus,—at least, for the moon's image.

In a letter to Col. Sabine, written in April last, Sir John Herschel strongly recommended that daily photographic representations of the sun should be made in some observatory, or

rather in several, with a view to an historical record of the spots.
The Kew Committee immediately entertained the suggestions of
Sir John Herschel, and, after they had made some inquiries in
regard of the probable cost of the necessary apparatus, came to
the conclusion that the proposal should be submitted to the Royal
Society, with a recommendation for its adoption.   The Council of
that Society ultimately decided that a photographic observatory
should be erected in connexion with the Kew Observatory, and,
with the concurrence of Mr. Oliveira, placed at the disposal of the
Kew Committee the sum of 150*l.*, which that gentleman had
liberally offered to the Royal Society, in aid of any scientific
object which it might deem desirable to promote.
        The Kew Committee having subsequently intrusted the car-
rying out of Sir John Herschel's views to Mr. De La Rue, one of
its members, that gentleman, after deciding on a plan, engaged
the services of Mr. Ross (well known for his success in the manu-
facture of telescopes and photographic lenses) for the construction
of the telescope and stand, which are now progressing, and which,
it is expected, will be erected in the course of three months.
        The diameter of the object-glass will be 3·4 inches, and its
focal length 50 inches; the image of the sun will be 0·465 inch,
but the proposed eye-piece will, with a magnifying power of 25·8
times and focal length x, increase the image to 12 inches, the
angle of the picture being about 13° 45′.   The object-glass will
be under-corrected in such a manner as to produce the best prac-
tical coincidence of the chemical and visual foci.*   The eye-piece
will consist of two nearly achromatic combinations, their forms,
foci, and focal lengths, to be arranged upon the basis of the photo-
graphic portrait lens, the conditions being nearly similar.
        It is contemplated to form the system of micrometer-wires on
a curved surface; and it may ultimately be found to be advan-
tageous also to curve the photographic screen, as the small
curvature necessary, namely, about two-tenths of an inch, will
present no mechanical difficulties.   As in practice it may possibly
be found desirable not to produce the sun's image with too great
rapidity, a provision is contemplated for the absorption of some of
the most energetic active rays by the interposition of coloured
media of different tints.
        The telescope being for a special object, it will have no appli-
ances, except such as appertain exclusively to that object, so that
the only means provided for *viewing* the sun will be through the
finder intended for facilitating the adjustment of the sun's image
in position, as regards the micrometer.   The polar axis will be
furnished with a worm-wheel and clock-work driver, and the
declination axis with a clamping circle.   A shutter for covering

* Mr. Ross has found that if for the greatest intensity of vision, in common
lenses, the ratio of the dispersive powers of the two media is 0·65, that the
chemical and visual foci will coincide but practically when with the same media
the ratio is altered to 0·60; the media he uses being Pellatt's flint and Thames
plate.

the object-glass and capable of being rapidly moved by the observer, will be so contrived as to be under his command, whether he be, at the time, near the object-glass or near the screen, eight feet distant.

The telescope will be placed in an observatory, twelve feet in diameter, and provided with a revolving roof; adjoining the observatory, a small room for chemicals will be constructed, so as to facilitate the fixing of the pictures.

The attention of this Society has been directed with interest for several years to the labours of Professor Hansen, and especially to the two greatest works which he has undertaken,— namely, to the formation of new solar and lunar tables. The former of these works has been completed, and the solar tables have been published within the last year; and, as it is probable that many of our members are as yet but imperfectly acquainted with them, a few words on their construction may not be unacceptable.

Throughout this laborious work M. Hansen has been assisted by M. Olufsen, whose name appears on the title-page as one of the authors, and who has contributed elements (for example, the obliquity of the ecliptic) from his own researches.

The elements used in the construction of the tables are based on all the observations available for the purpose which have been made at Greenwich and Königsberg; and a table is added of comparisons of tabular places with the observations made at those observatories in the series of years from 1820 to 1843, which shows that the observations are completely represented within the limits of probable error by the tables.

M. Hansen proposes to give in a separate memoir the details of the calculations by which the various expressions explicitly given in the introduction have been formed, though the introduction, in a very lucid manner, explains the adaptation of these formulæ to the tabular arrangement.

One most important addition in these tables is the introduction of the right ascensions and declinations of the sun, as well as the latitudes and longitudes, for in the daily practice of astronomy it is these quantities which are needed most constantly, and in the formation of ephemerides the advantage derived from them will be found to be very great.

M. Hansen states the objects which he proposed to himself in the arrangement of the tables to be the following :—

1. That they should not only give the tropical apparent longitude, the logarithm of the radius vector, and the latitude of the sun, but also *immediately* the right ascension and declination, because at the present time the latter are of more frequent use in astronomy than the former.

2. To add the tables by which are obtained the mean right ascension, the equation of time, the reduction of the tropical ap-

parent longitude to sidereal longitude, the diameter, the parallax, &c.

3. To construct the tables in such a way that they should give the quantities mentioned above with the same exactness as if they had been calculated immediately by the formulæ.

This great work is, undoubtedly, a valuable boon to modern astronomy; it is a work which has been long desired, and which undoubtedly combines the most remarkable combination of analytical skill and excellence of observations which have ever heretofore been brought to bear on any planetary tables whatever.

Since the last Annual Report was read Admiral Smyth has published his account of the Mediterranean, a work which, considered as an accompaniment to his charts by a nautical surveyor, is probably unique. As a manual of suggestions for the educated seaman, in his inquiries into the history, literature, remains of antiquity, commercial state, hydrography, meteorology, geology, and ichthyology of this most celebrated of all the waters on our globe, Admiral Smyth's work will at once take its place among the books of every traveller. The history of the charts of the Mediterranean will secure it a place in the hydrographer's library of research. To the astronomer it will have the specific value of giving power of ready reference to very many points of physics and geography, as they occur in connexion with the history and literature, as well as the application, of his science. And the scholar and the philologist will find the benefit of many elucidations of points of difficulty in his own pursuits. A general work on the Mediterranean can hardly fail to meet a want, now and then, in each and every branch of knowledge; and, in the present instance, no more of description than is necessary for sufficient announcement will justify the Council in congratulating the Society on the appearance of such a work under the name of one of our Fellows.

Among the publications of the past year which bear upon our branch of science the Council notice with much satisfaction a little work from the pen of Mr. James Breen, senior assistant at the Cambridge Observatory, entitled, *The Planetary Worlds.* One of our most excellent assistants, whose daily duties are sufficiently onerous to excuse the exertion, has taken advantage of his opportunity of access to a good astronomical library, and to one of the finest telescopes in Europe, and has found time to present the public with an interesting collection of descriptive matter, in a simple form, and of a kind frequently inquired for. The numerous woodcuts with which the volume is illustrated are hardly of a quality of execution to do justice to the letterpress, but, with a little correction by the reader's judgment, will be found materially to assist him in forming an idea of the various appearances seen by modern observers under bright optical aid.

Professor Hansen has recently communicated to the President a detailed account of his progress in the construction of the Lunar Tables which for some time past have occupied so much of his attention. By a comprehensive discussion of the Greenwich Observations of the moon he deduced the corrections of the elements of the lunar orbit. A comparison instituted by him between the observations extending from 1824 to 1850, and the results of the theory thus improved, exhibits a most satisfactory accordance. The early observations of Bradley are also well represented by the theory, although, as might be expected, the agreement is not so close as in the case of the more modern observations. In the Tables which Professor Hansen is constructing from his theory the arguments are expressed, not in arcs of the circle, but in time, the unit of the argument being the mean time which elapses between two successive culminations of the moon. This unit is especially adapted to a comparison of the results of theory with a series of meridian observations of the moon; but when the question refers to the calculation of an ephemeris of the moon's place for fractions of a day, similar to that given in the *Nautical Almanac*, its advantages are not so obvious as Professor Hansen candidly admits.

The outstanding differences between observation and theory appearing to Professor Hansen to indicate the necessity of an enlargement of the coefficients of several of the most important inequalities in the moon's longitude, he was led to inquire into its origin, and he found that it might be to a great extent accounted for by supposing that the centre of gravity of the moon did not coincide with its centre of figure. This result is embodied in a very remarkable theorem, to which he has been conducted by his researches on the subject.

A total eclipse of the sun, although of extremely rare occurrence, is accompanied by phenomena of a highly important nature in regard to various questions connected with the physical constitution of the sun. An event of this kind is, therefore, always watched with unusual interest by the astronomer. On the 30th of November, 1853, there happened a total eclipse of the sun; but, unfortunately, it did not admit of being observed by the astronomers of Europe, the obscuration having been confined to the Pacific Ocean and a portion of the west coast of South America. The Chilian Republic, however, with praiseworthy zeal, despatched M. Moesta, the Director of the Observatory of Santiago, to a suitable station in Peru, near the central track of the moon's shadow; and a complete observation of the various phenomena of the eclipse was made by that astronomer, of which an account appeared in the *Monthly Notices* for June last. One of the features of this eclipse, which seems more especially worthy of notice, consisted in an interruption of the contour of the luminous ring usually seen around the dark body of the moon, arising from two apertures

contiguous to the moon's limb, through which the dark ground of the heavens was visible.

After the death of Dr. Petersen, the publication of the *Astronomische Nachrichten* was carried on, provisionally, by Professor Hansen. Professor Peters has since been appointed to the Altona Observatory, which includes, in its understood duties, the editorship of the *Nachrichten.* Under his able management we may feel sure that this valuable journal will maintain its high reputation.

In the past year some difficulties have arisen in the transmission of the *Nachrichten* to English subscribers, *by post,* and there have been many vexatious stoppages and overcharges. An application from the President to the proper authority has at last put an end to these troublesome scruples of the subordinate officers, which were quite at variance with the liberal views of the heads of the establishment. We have to acknowledge, with thanks, that Mr. Rowland Hill has assisted us on every occasion to overcome the official fiction, and with great readiness.

The Fellows are aware that, according to our present system of publication, observations and ephemerides are for the most part turned over to the *Nachrichten,* as the best deposit for so much of practical astronomy, as does not appear in special publications. Amateur observers know this, and find the convenience of a means of communication which embraces the civilised world. Perhaps those friends of astronomy, who are not observers, may be told that there is scarcely any way in which they can encourage the science more effectually, than by contributing to a publication which is one of the main supports of astronomy, and a principal organ in its extension and improvement.

Papers read before the Society from February 1854
to February 1855.

1854.
Mar. 10. On a Mode of Mounting a Telescope Equatoreally. Mr. Rothwell.

Extracts from a Letter to the Astronomer Royal. M. O. Struve.

On the Physical Constitution of the Moon. Mr. Nasmyth.

Observations of a Comet. Mr. Drury.

On the Shadow of *Saturn* on the Ring. Mr. Hippisley.

Observations of Comet III. 1853. Mr. Heath.

Observations of Van Arsdale's Comet. Mr. Bond.

Discovery of a new Planet. Mr. Marth.

On Observing Spots on the Sun. Mr. Carrington.

Observations of Solar Spots. Capt. Shea.

Account of Hartwell Rectory Observatory. Rev. C. Lowndes.

On the Orbit of 70 *Ophiuchi.*　Mr. E. Powell.

On the Resistance of an Ethereal Medium, &c.　M. Angström.

Observations of Comet IV. 1854.　Mr. Hind.

On the Difference of Longitude between Brussels and Greenwich, as ascertained by Galvanic Signals.　The Astronomer Royal.

On a Method of finding Greenwich Mean Time at Sea. M. Rümker.

Extract of a Letter from Prof. Secchi.

Discovery of a new Planet.　M. Goldschmidt.
　　Do.　　　Do.　　　M. Chacornac.

Account of Observatory and Observations of Satellite of *Neptune.*　Mr. Lassell.

Notes on Chinese Astronomy.　Mr. Williams.

On the Figure of the Moon.　M. Hansen.

On the Construction of Lunar Tables.　M. Hansen.

On the Eclipse of Nov. 30, 1853.　Capt. Shea.

On a new Eye-piece for Solar Observations.　Mr. Hodgson.

Dec. 8.　On the Orbit of *a Centauri.*　Mr. E. B. Powell.

Investigation of Foucault's Pendulum Experiments. Lieut. Ashe.

On Shepherd's Galvano-Magnetic Regulator.　Mr. Ellis.

Telescopic Appearance of *Venus* at Inferior Conjunction. Dr. Drew.

On the Origin of the Attempts to deduce Invariable Standards of Measure from Physical Principles.　Mr. Grant.

On the Satellites of *Neptune.*　Mr. Hind.

On the Satellites and Mass of *Uranus.*　Mr. Hind.

Note respecting the Pendulum Experiment.　The Astronomer Royal.

On a Phenomenon relating to the Hill of Santa Lucia. Dr. Moesta.

1855.
Jan. 12.　On Rating Chronometers by Lunars.　Capt. Toynbee.

On the Orbit of *r Coronæ Borealis.*　Mr. E. B. Powell.

Observations of Zodiacal Light.　Mr. Burr.

On the Telescopic Appearance of *Saturn.*　Rev. W. R. Dawes.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Her Majesty's Government.
Royal Society of London.
Royal Society of Edinburgh.
Royal Geographical Society.
Royal Asiatic Society.
Royal Institution.

Royal Irish Academy.
Geological Society.
Linnean Society.
Society of Arts.
The Photographic Society.
Cambridge Philosophical Society.
The Philosophical Society, Liverpool.
The Philosophical Society, Manchester.
British Association.
University College, London.
Institute of Actuaries.
Corporation of Glasgow.
Hon. East India Company.
· The Registrar-General.
The Superintendent of the Nautical Almanac.
The Radcliffe Trustees.
L'Académie National des Sciences de l'Institut de France.
L'Académie des Sciences de Dijon.
Société des Antiquaires de Picardie.
Royal Academy of Munich.
Royal Academy of Berlin.
Royal Academy of Brussels.
Royal Academy of Göttingen.
Royal Academy of Madrid.
The Society at Genoa.
The Academy of Science, Naples.
The American Philosophical Society.
American Academy of Arts and Sciences.
The Smithsonian Institution.
The Franklin Institute.
The National Observatory, Washington.
The Observatory at Brussels.
The Observatory at San Fernando.
The Editor of the Athenæum Journal.
The Editor of the Literary Gazette.

J. C. Adams, Esq.	M. Faye.
G. B. Airy, Esq.	M. Gautier.
C. Babbage, Esq.	Dr. B. A. Gould.
Prof. A. D. Bache.	R. Grant, Esq.
Admiral Sir F. Beaufort.	W. Gray, Esq.
Jas. Bedford, Esq., Ph. D.	M. Grunert.
W. Brown, Esq.	J. Herapath, Esq.
Sig. Carlini.	Luke Howard, Esq.
Rev. Prof. Challis.	Capt. Jacob.
M. Daussy.	Dr. Lee.
Prof. De Morgan.	M. Liouville.
S. M. Drach, Esq.	M. Von Littrow.
Dr. Drew.	L'Abbé Moigno.
Prof. Encke.	Don C. Moesta.

Dr. Peters.	Thos. Tate, Esq.
M. Plantamour.	R. Taylor, Esq.
W. Pole, Esq.	Sir W. C. Trevelyan.
Prof. Quetelet.	M. Villarceau.
Lieut. H. Raper.	H. Warburton, Esq.
W. Rathbone, Esq.	J. A. Welton, Esq.
M. Reslhuber.	T. T. Wilkinson, Esq.
Capt. Shadwell.	Lord Wrottesley.
Rev. R. Sheepshanks.	Mr. J. Williams.
J. J. Sylvester, Esq.	

Address delivered by the President, G. B. Airy, Esq. F.R.S., Astronomer Royal, on presenting the Medal of the Society to the Rev. William Rutter Dawes.

I have now to state to you, Gentlemen, that the Council have this year awarded the Medal of the Society to the Rev. William Rutter Dawes for his various astronomical works; and I take the opportunity of accompanying this statement with the following remarks.

A science so extensive as ours will necessarily present for the consideration of the Council, in the adjudication of the Medal, subjects of very different classes. We have sometimes to examine the difficult mathematical investigations of gravitational astronomy, and sometimes the less profound, though more bulky, calculations connected with the ordinary conduct of an observatory. We must sometimes balance the formal and dry labour of meridional observations against the less regular, but more interesting, work of extrameridional and micrometrical investigation. We must not estimate lightly the successful inventor of astronomical instruments; still less must we omit to recognise, in a science where everything depends on the connexion of past, present, and future, the important services of the literary astronomer. Finally, while we may sometimes hold ourselves bound to consider a single work of short duration, but of great merit, as demanding our best acknowledgment; in other cases we are equally required to extend our view over a long series of years, and to appreciate the duration as well as the specific excellence of an astronomer's labours. Thus it will usually happen that the subjects to which our Medals are adjudged, in the course of several successive years, have little similarity in their character or extent. The subject of the present medal differs in some respects from any which has been brought before you for a considerable time.

Mr. Dawes has been known for nearly a quarter of a century as a zealous private cultivator of astronomy, by extra-meridional and micrometrical observations. Distinguished as Mr. Dawes has been by an extraordinary acuteness of vision, and by a habitual,

and (as I may say) contemplative precision in the use of his in-
struments, his observations have commanded a degree of respect
which has not often been obtained by the productions of larger
instruments. The first of his observations were made with a
5-feet telescope, of about 3 inches aperture; a few with a 7-feet
Newtonian: subsequent observations have been made with Mr.
Bishop's equatoreal and with a fine 6-inch Munich telescope. A
part of these observations have been published in Mr. Bishop's
volume, and a part in the *Memoirs* and *Monthly Notices* of this
Society.

A large portion of the observations consists of measures of
double stars. The first are two measures of ζ *Cancri* in 1831.
These are followed by measures of 121 stars from 1831 to 1834,
of 100 stars from 1834 to 1839, and of about 250 stars (in Mr. Bi-
shop's volume) from 1839 to 1844. Of these stars, a great propor-
tion are very close couples; in fact, Mr. Dawes seems to have been
able successfully to grapple with nearly all the objects which have
most severely tried the optical powers of the best foreign instru-
ments. There are also various observations of occultations (one
the occultation of a star by *Jupiter*); and I am unwilling to pass
over the nearly singular observation (given to the world, I think,
in another channel of communication) of *Jupiter* without any
satellite visible.

To the astronomer who delights in the investigations based
upon numerical measures, the observations to which I have re-
ferred will appear the most valuable. Yet, if I may venture a
prediction, there are other observations by Mr. Dawes which will
be cited more frequently: I allude to those on the physical ap-
pearances of the planet *Saturn*. First, the observations made in
1843 on the apparent division in *Saturn's* outer ring. Secondly,
the phenomena observed in 1848, from June to December, at the
disappearance of the ring. Thirdly, the independent discovery by
Mr. Dawes, in 1850 November, of the dusky ring, and his sub-
sequent observations on that wonderful appendage of *Saturn*.
The last is, indeed, an admirable instance of what may be done
with a comparatively small telescope, when it is used by an ardent
intellect to sharpen the powers of an eye naturally acute. The
astonishing phenomena of the total solar eclipse of 1851 were also
observed by Mr. Dawes, who made the voyage to Sweden for that
purpose.

I have thus far confined my notices to observations only, but
I must not omit to remark, that astronomy is indebted to Mr.
Dawes for several suggestions of extensive practical use. One of
these is a series of proposals for determining a standard of optical
power, and for laying down a scale of the magnitudes of stars.
Another is a mode of observing the spots of the sun, which has
given a more profound insight (I use the word in its geometrical
and not in its metaphorical sense) into these remarkable craters
than any preceding method.

I have recounted in detail these different astronomical works

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of Mr. Dawes, because it is on the assemblage of them that his claims to your honourable notice are based. Perhaps, if any one of them, or any one class of them, had been submitted to your Council, there might have been among the members of that body a disposition to examine carefully whether other single works might not be found presenting equal demands for their attention. But, in reviewing the assemblage of works — all unexceptionable in quality, of different kinds, continued through a long series of years — the Council have had no such misgiving. They feel that they have had ample grounds for recognising in the person of Mr. Dawes one of the best friends of astronomy, and in the works of Mr. Dawes, not only some of the most valuable contributions to astronomy, but also some of the most valuable patterns to future observers; and they are persuaded, that the approbation of the Society will accompany the Council's award of the Medal.

The President then delivering the Medal to Mr. Dawes, addressed him in the following terms: —

Mr. Dawes, — In the name of the Council of the Royal Astronomical Society I present you with the Medal of the Society, in acknowledgment of your long-continued devotion to astronomy, of your numerous contributions to various divisions of that science, of the remarkable excellence of your observations, and the recognised value of your suggestions. And I trust that there remain to you many years of health and happiness, many years of enjoyment of your favourite science, and many discoveries tending to the material and intellectual advance of astronomy.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President :

MANUEL J. JOHNSON, Esq., Radcliffe Observer.

Vice-Presidents :

G. B. AIRY, Esq. M.A. F.R.S. Astronomer Royal.
AUGUSTUS DE MORGAN, Esq.
JOHN LEE, Esq. LL.D. F.R.S.
Admiral W. H. SMYTH, K.S.F. D.C.L. F.R.S.

Treasurer :

GEORGE BISHOP, Esq. F.R.S.

Secretaries :

WARREN DE LA RUE, Esq. F.R.S.
Captain R. H. MANNERS, R.N.

Foreign Secretary :

JOHN RUSSELL HIND, Esq. Superintendent of the Nautical Almanac.

Council :

ARTHUR KETT BARCLAY, Esq. F.R.S.
RICHARD C. CARRINGTON, Esq.
Rev. GEORGE FISHER, M.A. F.R.S.
JAMES GLAISHER, Esq. F.R.S.
ROBERT GRANT, Esq. M.A.
Rev. ROBERT MAIN, M.A.
Rev. BADEN POWELL, M.A. F.R.S.
WILLIAM RUTHERFORD, Esq. LL.D.
Rev. RICHARD SHEEPSHANKS, M.A. F.R.S.
WILLIAM SIMMS, Esq. F.R.S.

ROYAL ASTRONOMICAL SOCIETY.

M. J. JOHNSON, Esq. President, in the Chair.

J. B. Dancer, Esq., Manchester ;
Wentworth Erck, Esq., 27 Herbert Place, Dublin ; and
R. J. Mann, M.D., 7 St. Boniface Terrace, Ventnor,
were balloted for and duly elected Fellows of the Society.

Death of Professor Gauss.

A letter was read from M. Hausmann, Secretary of the
Royal Society of Sciences of Göttingen, announcing the death of
this distinguished individual, on the 23d ultimo, in the seventy-
eighth year of his age.

Account of Operations connected with the Advancement of Commercial Astronomy in Australia. By Robert L. J. Ellery, Esq.

(*Extracts of a Letter to the Astronomer Royal.*)

" Although not a member of the Astronomical Society, I take
the liberty of communicating to you, as President of that body,
the following information, deeming that the interest felt by the
Society in the progress of science may prove a sufficient excuse
for addressing them on so trivial a matter.

" In June 1853, a time-ball signal—visible to the shipping in
Hobson's Bay—was erected on the mast or flagstaff, Gellibrand's
Point.   Soon after its erection, I was appointed to take the neces-
sary observations for the regulation of this signal.   At that time,
two chronometers (neither of them first-class instruments), and two
indifferent sextants, with a small artificial horizon, composed the
staff of instruments.   By using the system of equal altitudes on
every possible opportunity, I ascertained the rate of one chro-
nometer to be sufficiently reliable to enable me to give the time,

even through a few days of cloudy weather; but the sudden and great changes of temperature act as a severe test to a chronometer's performance.

"In August, a small transit instrument was offered for sale, and bought by Government, with the works of a new regulator clock by Evans of Birmingham. Authority was given me to get a room built for the instrument and clock, which was done, attached to my quarters. The instrument was one of Spencer and Browning's 23-inch, on iron stand. Having some fine stone close at hand, I got some blocks cut, and a very substantial pier built. The building being of wood, the foundations for both transit-pier and clock had to be quite free from the building. The works of the clock were put in a case I got made here, and was mounted on a stone pier, built up about an inch above the level of the floor, with a large plank as a support at the back, built in with the foundation. The room had its meridional opening from horizon to horizon,—the south opening having a sea horizon, the north highlands at the back of Melbourne. After much trouble in adjusting the transit from the constant settling of the pier, from bad cement having been used, I got it into working order: got up a meridian mark, about half-a-mile away, consisting of three parallel vertical lines, finely marked with black paint on a white ground; and after that it soon became so steady as to only require occasional levelling. The clock, whose compensation was the mercurial one, after some little trouble, went very well and steadily in all the great extremes of temperature we are subject to (the temperature often has varied $45°$ from noon till midnight).

"With these instruments the time-signal has been given daily, Sundays excepted, up to February 1854, when a few instruments which had been ordered by the Colonial Government arrived, consisting of 30-inch transit by Potter (late Bates), regulator-clock, mercurial pendulum by Frodsham, sextant by Potter. These instruments were put into use at once; a new pier with a broader foundation was built for transit, and the old clock was removed to a room we use for chronometer-room, where a great many chronometers are brought for rating, &c. Up to this present time no other addition has been made, with the exception of one or two plain meteorological instruments. The clock by Frodsham is performing beautifully, having a steady rate of $-0^s·3$ a-day, with scarcely any perceptible deviation for temperature. Several times I have noticed, when the barometer was at 30·20—30·25, a little more loss; but on the whole it has performed exceedingly well. The work that I have done with these instruments is as much as could be well done. The time-keeping is, of course, the main object at present; but, in addition, I am tabulating a regular list of zenith and circumpolar stars (of course of right ascension only, as the instrument has only a setting circle reading to minutes). I have, I think, determined the longitude of the observatory pretty closely, from the mean of

35 sets of moon-culminations. Its previously assigned position was 9ʰ 39ᵐ 41ˢ·8 E., 37° 52′ 42″ S.; but I make the longitude, 9ʰ 39ᵐ 40ˢ·0 by the mean of 35 culminations. The assigned latitude I have always found correct. The previous position was, I believe, given by Captain Stokes, of one of her majesty's surveying vessels.

" The time-ball is a frame-work ball covered with canvass painted black, and is hoisted to the masthead by halyards, which halyards, when the ball is hoisted, are attached to a trigger below, and by a slight pull on which the halyards are released the ball drops. It is rather a primitive arrangement, but answers pretty well. · Since the chief station of the electric telegraph has been completed at Melbourne, a time-ball has been placed on the tower of that building for the benefit of the city of Melbourne, and it is dropped at the same instant as the one here by means of the telegraphic wire. As yet no coil magnets are used for releasing the detents, as we have not been able to get them made yet; but I am only now waiting for authority to get the connexion as complete as possible. As it is now, I have to take a previously compared chronometer to the Flagstaff, a distance of about 50 yards from the observatory, and by the side of the trigger of the time-ball is a little brass *circuit closer* (the whole line being in complete circuit from 5 minutes to 1 o'clock, except at the Flagstaff, where the signal is given from). The ball in Melbourne being hoisted at the same time it is here— 3 minutes before one—I connect the circuit several times to see all is right; each connexion is made known to the telegraph operator in Melbourne by the deflexion of a small needle indicator; he then stands in readiness with his hands on the trigger, and 5 seconds before one—I give seconds beats of the indicator—by connecting each second at the fifth beat both balls are released; and, from repeated watching, no perceptible interval in their dropping is perceived. The time-balls are about 8 miles apart: the one in Melbourne is a little to the eastward of the observatory, —about 2 seconds' time, I think. The exact instant of the drop of the Williamstown time-ball is marked by my only assistant, my wife; and if any error occurs, from wind or otherwise, it is thus accurately accounted for, and published in the daily papers.

" It is proposed to erect time-balls on the electric telegraph stations, which are now in course of erection at Geelong and Port Phillip Heads, so as to make every use of the observatory possible in giving mean time to different parts of the colony. The plan for these time-balls I have given myself, and will be constructed so as to be dropped by direct galvanic current without any manual intervention; and I wish to adopt the same means that you have done with regard to the time-ball at Deal (I think it is Deal),— viz. an apprisal by the drop of the signals themselves that they have dropped correctly. In addition to time-balls here, since June last another means has been adopted as a time-signal,—viz. the light of the lighthouse on Gellibrand's Point. It having been

represented to the chief harbour-master that on account of the great crowd of shipping those lying towards the opposite shores could not see the time-ball, and begging·that some means might be adopted to make the signal more generally seen, the harbour-master had an apparatus fitted to the inside of the lantern, which obscures the light by releasing one trigger, and shows it again suddenly by releasing another. This was his own arrangement, and my instructions were to obscure the light at 2 minutes to 8 P.M., and suddenly show it at 8 o'clock, the latter being the true signal, any error of which would be published in the daily papers. It answers very well, but is very inconvenient, climbing the lighthouse-tower with a chronometer in hand. The master-mariners think it a great convenience, as it allows them to attend to the rating of their chronometers themselves, for business often keeps them on shore during the day. If this is continued, I must get some better plan devised: the telegraph wires could be used here too.

" The sum of 2500*l.* was voted by the Legislative Council for the building a stone observatory at Williamstown; this has not been done yet, but the Governor has placed in the estimates for next year the sum of 660*l.* for the purchase of astronomical and meteorological instruments, according to a list of what would absolutely be required that I was instructed to send in. The chief of these instruments were,—a transit circle, by Troughton and Simms; a 5 or 6-feet equatoreal by Troughton and Simms, or Ross; a portable transit collimator, &c.; with a small equipment of good meteorological instruments. In the ultimate purchase of these instruments, perhaps you might give some advice, from your great experience in such matters. At all events, I shall take the liberty of informing you when the orders are sent home for these instruments, in case you might feel any interest in guiding the purchase of them. I had omitted to say, that since December last I have kept a regular journal of three-hourly meteorological observations,— at least, so far as my limited staff of instruments allowed me, having only a simple, but very good pediment barometer, Zambra's thermometer, and aneroid barometer. Having neither rain-guage, anemometer, or hygrometer, of course my observations are very limited; but, such as they are, they represent some interesting facts with regard to Australian meteorology.   .

" *Observatory, Gellibrand's Point,*
    Williamstown, Victoria."

Account of the Steps recently taken by Her Majesty's Government for Promoting the Regular Observation of Meteorological Phenomena at Sea. By Capt. Robert Fitzroy, R.N.

" The importance of accumulating meteorological observations,

and tabulating them methodically, for the purpose of future, rather than immediate investigation, having been urged by the Royal Society, while the practical benefits arising from such collections, even at the present time, were proved by the direct consequences of Maury's extensive labours, Her Majesty's Ministers agreed to establish an office under the Board of Trade for receiving and tabulating all such observations made at sea.

"It was considered that much information might be compiled with respect to currents, as well as winds, which might be made more generally known to those interested in the passages of ships across the ocean ; and that the sooner such authentic compilations could be made generally available, the greater would be their value. It was, moreover, pronounced to be necessary that instruments of a reliable and understood nature should be alone employed ; that they should be carefully tested and vigilantly guarded from accidental causes of error.

"To meet these objects, an estimate of probable expenses was submitted to Parliament, and the sums proposed were voted, namely, 2000*l.* for the Mercantile Marine and 1000*l.* for her Majesty's ships.

"Soon afterwards an officer was appointed to execute the duties of the Meteorological Office, to be subsequently assisted by a few subordinates ; but some time elapsed before instruments of the peculiar kind deemed proper by a Committee of the Royal Society could be finished, and an office appropriated for the object in view. Now the preliminary arrangements are made, and the Meteorological Office of the Board of Trade is open at No. 2 Parliament Street.

"A certain number of selected ships of the Mercantile Marine, and all those of her Majesty employed in long or distant voyages, are, or soon will be, engaged in making exact observations with instruments supplied under the authority of the Board of Trade (duly tested and compared), and in registering the apparent results according to forms settled at the Brussels Conference of 1853, slightly modified, however, so as to suit present convenience.

"The estimates sanctioned by Parliament are sufficient to provide sixty merchant-ships and forty men-of-war with the necessary meteorological instruments (namely, barometers, thermometers, and hydrometers), in addition to the nautical instruments usual at sea ; to pay office expenses and salaries (including allowances to agents at outports) ; and to provide the necessary registers. A captain in the navy is in charge of the office. Four subordinates are to assist him, and there are agents appointed at the principal ports to communicate personally with the owners, captains, and officers of ships.

"Liberally supplied by the United States Government, Maury's Sailing Directions and Charts are distributed gratis among those who undertake to record observations satisfactorily, and send them to the Board of Trade. Marks, expressive of distinction, are to be annexed to the names of approved contributors to meteorology

in the Mercantile Navy List, and other encouragements are contemplated.

"Every exertion will be made at the office, not only to discuss and tabulate valuable observations, but to digest and render available, as soon as possible, such information as may tend immediately to the improvement of navigation.

"*February 8, 1855.*"

On the Application of Photography to Astronomical Observations.

(*Letter from Sir John F. W. Herschel to Colonel Sabine.*)

"I consider it an object of very considerable importance to secure at some observatory, and indeed at more than one, in different localities, daily photographic representations of the sun, with a view to keep up a consecutive and perfectly faithful record of the history of the spots. So far as regards the general delineation of the whole disk, and the marking out on it, in reference to the parallel to the equinoctial passing through its centre, the places, sizes, and forms of the spots, there would need, I should imagine, no very powerful telescope,—quite the contrary; but it should be equatoreally mounted, and ought to have a clock motion in the parallel. The image to be impressed on the paper (or collodionized glass) should be formed not in the focus of the object-lens, but in that of the eye-lens, drawn out somewhat beyond the proper situation for distinct vision (and always to the same invariable distance to insure an equally magnified image on each day). By this arrangement, a considerably magnified image of the sun, *and also of any system of wires* in the focus of the object-glass, may be thrown upon the ʻfocussing-glassʼ of a camera-box adjusted to the eye-end of a telescope. By employing a system of spider-lines, parallel and perpendicular to the diurnal motion, and so disposed as to divide the field of vision into squares, say of 5′ in the side, the central one crossing the sun's centre (or rather as liable to no uncertainty, one of them being a tangent to its lower or upper limb), the place of each spot on the surface is, *ipso facto*, mapped down in reference to the parallel and declination circle, and its distance from the border, and its size measurable on a fixed scale.

"If large spots are to be photographed specially with a view to the delineation of their forms and changes, a pretty large object-glass will be required, and the whole affair will become a matter of much greater nicety; but for reading the daily history of the sun, I should imagine a 3-inch object-glass would be ample.

"The representations should, if possible, be taken daily, and the time carefully noted. As far as possible, they should be taken at the same hour each day; but in this climate, a clear interval, occurring when it may, had better be secured early in the day.

"Three or four observations in tropical climates, distant se-

veral hours in longitude (suppose 3, at 8^h distance in longitude), each recording at, or nearly at noon, would, when the results were assembled, keep up a continuous history of the solar disk.

" With regard to proper preparation of paper, or the use of collodion acid, the photographic art is now so much advanced, that no difficulty can arise in fixing upon fitting preparations, or the manipulations necessary for multiplying them. But it would be very requisite that many impressions of each day's work should be taken and distributed, and an intèrchange kept up among observers.

"*April* 24, 1854."

Observations of Comet I. 1855. By Dr. Donati.

(*Communicated by Mr. Drach.*)

1855.	Florence M.T.	Comet—Star.		Comet's Apparent	
		Δ α.	Δ δ.	R.A.	Decl.
	h m s	m s	′ ″	° ′ ″	° ′ ″
Feb. 15	17 9 8·1	+4 7·02	−17 34·0	16 30 57·37	−28 12 31·8*
	17 37 38·5	−4 55·76	+ 1 57·7	16 30 59·30	−28 12 30·3†

      * 5 comparisons with *a.*   † 3 comparisons with *b.*

Adopted Apparent Places of Comparison Stars.

		R.A.	Decl.		
		h m s	° ′ ″		
a	Feb. 15	16 26 50·35	−27 54 57·8	Piazzi 113	Hora xvi.
b	—	18 35 55·06	−28 14 28·0	— 159	—

The comet was very faint.

On the Constitution of the Atmosphere, upon which Laplace's Table of Astronomical Refractions is founded. By Sir J. W. Lubbock, Bart. F.R.S.

" I have elsewhere given a table showing the constitution of the atmosphere, upon which Ivory's table of refractions, and also that upon which my own table of refractions, is founded.

" I have calculated the following table from the expressions given by Laplace in the fourth volume of the *Méc. Cél.* p. 265 :—

Height in Miles.	Laplace.		
	Pressure, inch.	Temp. Fahr.	Density.
0	29·94	32·00	1·00000
1	24·33	6·92	·85744
2	19·56	−14·14	·72283
3	15·57	32·60	·60010
4	12·31	47·24	·49114
5	9·60	60·23	·34709
10	2·54	111·21	·11743
15	·60	120·00	·02937
20	·13	139·70	·00674

" The pressures in Laplace's theory are up to the height of five miles *less* than those of Ivory or my own, so that in calculating heights by means of the barometer, Laplace's formula would give the heights rather greater ; the difference would be, roughly,

$$
\begin{aligned}
&\text{At a height of 1 mile,} \quad &263 \text{ feet}\\
&\qquad\quad - \qquad\quad 2 \quad \dots\dots \; 519\\
&\qquad\quad - \qquad\quad 3 \quad \dots\dots \; 749\\
&\qquad\quad - \qquad\quad 4 \quad \dots\dots \; 898
\end{aligned}
$$

" The pressures in my atmosphere and that of Ivory within these limits may be considered as identical.

" The temperatures in Laplace's atmosphere diminish far more rapidly at first than in Ivory's atmosphere or mine, namely, at the rate of a degree of Fahrenheit for every 210 feet: in my atmosphere and in Ivory's they diminish at the rate of about 1° Fahr. for 350 feet.

" The density in Laplace's atmosphere is greater than in Ivory's or mine.

" The following table, which has been published elsewhere, is repeated here for the sake of comparison :—

Height in Miles.	Lubbock.			Ivory.		
	Pressure. inch.	Temp. Fahr.	Density.	Pressure. inch.	Temp. Fahr.	Density.
0	30·00	+ 50·0	1·00000	30·00	+ 50·0	1·00000
1	24·61	35·0	·84611	24·61	34·4	·84875
2	20·07	19·5	·71294	20·05	18·3	·71373
3	16·25	+ 3·4	·59798	16·22	+ 4·9	·59472
4	13·06	− 13·3	·49903	13·07	− 6·3	·49118
5	10·41	30·6	·41403	10·47	16·2	·40230
10	2·81	126·4	·14499	3·25	45·8	·13407
15	·45	240·6	·03573	·95	56·2	·04044
20	...	...	...	·28	59·4	·01175
22·35	...	−448·0	·00000			
30	...	...	...	·02	60·6	·00097

" It appears to me that accurate observations are still wanting to determine the rate at which the temperature diminishes with the altitude, and especially to decide whether the rate be greater at night than by day.   This is the more important as astronomical observations are chiefly made at night.   Where I reside in the country, the situation is somewhat elevated, and the climate is cold in consequence, as I believe, but the nights are much colder in proportion than the days."

Remarks on Mr. Powell's Elements of the Orbit of 70 *Ophiuchi.*
By Isaac Fletcher, Esq.

" The *Monthly Notices* for December last contain an interesting
and very able discussion by Mr. Powell of the orbit of this re-
markable star. So far as the agreement between the observed and
computed angles of position is concerned, Mr. Powell's orbit repre-
sents, with singular fidelity, the actual path of the companion
star. As Mr. Powell remarks, the agreement is less satisfactory
as regards the distances, and it is on this point that I am desirous
of making a few brief remarks.

" If Mr. Powell's orbit is correct, the distance of the two stars
is still increasing; and Mr. Powell is of opinion that the measures
of distance in existence are not sufficiently trustworthy to warrant
an alteration in the elements sufficient to reconcile the great dis-
crepancy between the calculated and observed distance for the
epoch of 1854. This discrepancy amounts to $0''\cdot538$.

" I can hardly believe that an error of such magnitude can exist
in the measured distance of so easy a star as 70 *Ophiuchi*, espe-
cially if that distance is the result of several nights' observations
by so experienced an observer as Capt. Jacob. Having carefully
examined all the observed distances of 70 *Ophiuchi*, I find the
individual results of each observer, as well as the united testi-
mony of all, concur in indicating an *increase* in distance up to the
epoch of 1849 or 1850, since which time it appears to me there is
evidence equally unequivocal of a *decrease* in that element. If this
be really the fact, I think, as Mr. Powell has pointed out, a slight
alteration in the position of the node, or of the periastre, would
reconcile the theoretical orbit with the actual fact. As I have
a pretty strong opinion that the distance is now diminishing, I
beg to submit a statement of all the measures of distance I am
acquainted with since 1850.

By Capt. Jacob.

Epoch		Distance	
Epoch	1850·571	Distance	6''86
—	1852·75	—	6·73
—	1854·081	—	6·365

" The above are taken from Mr. Powell's paper.
" In the last supplement to the *Notices*, however, Capt. Jacob's
result for 1854 is thus stated:

Epoch	1854·262	Distance	6''·51

Whichever value, however, is adopted, my agreement is not
weakened, viz., that Capt. Jacob's measures show a decided
decrease in distance.

<div align="center">

By Mr. J. F. Miller.

</div>

Epoch	1851·64	Distance	6·508
—	1852·71	—	6·466

The first of these results is derived from a mean of 32 measures, and the latter from a mean of 22.

<div align="center">

By myself.

</div>

Epoch	1850·66	Distance	6·459
—	1851·58	—	6·378
—	1852·63	—	6·362
—	1854·62	—	6·300

The first result is from a mean of 56 measures, the second from a mean of 126, and the third from a mean of 78, but the fourth is merely a single night's result, and therefore of little value.

" An inspection of the above measures shows that three different observers, working independently, have all obtained results which conspire to prove that the distance is now decreasing.

" If the distance is really increasing, it is, at any rate, extraordinary, that these results should indicate the contrary.

" In making these remarks, I am very far from wishing to cavil at Mr. Powell's elements.   On the contrary, I think that his orbit is decidedly the best that has yet appeared, but I think this able computer has hardly given to the measures of distance the weight to which they are entitled.   If the elements could be varied so as to represent the recent observed distances with tolerable precision, without introducing more serious differences in the angles, it would, I think, be more satisfactory.

" Mr. Powell's important researches have, however, indisputably established a fact which has been doubted in high quarters, — that the relative motions of the components of 70 *Ophiuchi* do follow the Newtonian laws of gravitation.

" *Tarn Bank, Jan. 24, 1855.*"

<div align="center">

On a Telescopic Appearance seen in the Moon.
By Robert Hart, Esq.

(*Extract of a Letter to the Astronomer Royal.*)

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" Since I wrote you last [respecting the two fiery-looking spots in the moon's disk] I have taken every opportunity of examining the same region of the moon while it was in shadow, and also during the early part of this moon, and most carefully when she was of the same age.   But there is no such appearance of any such light as was so visible on the night of the 27th December last, between 7 and 12 P.M. (the time that I observed her).
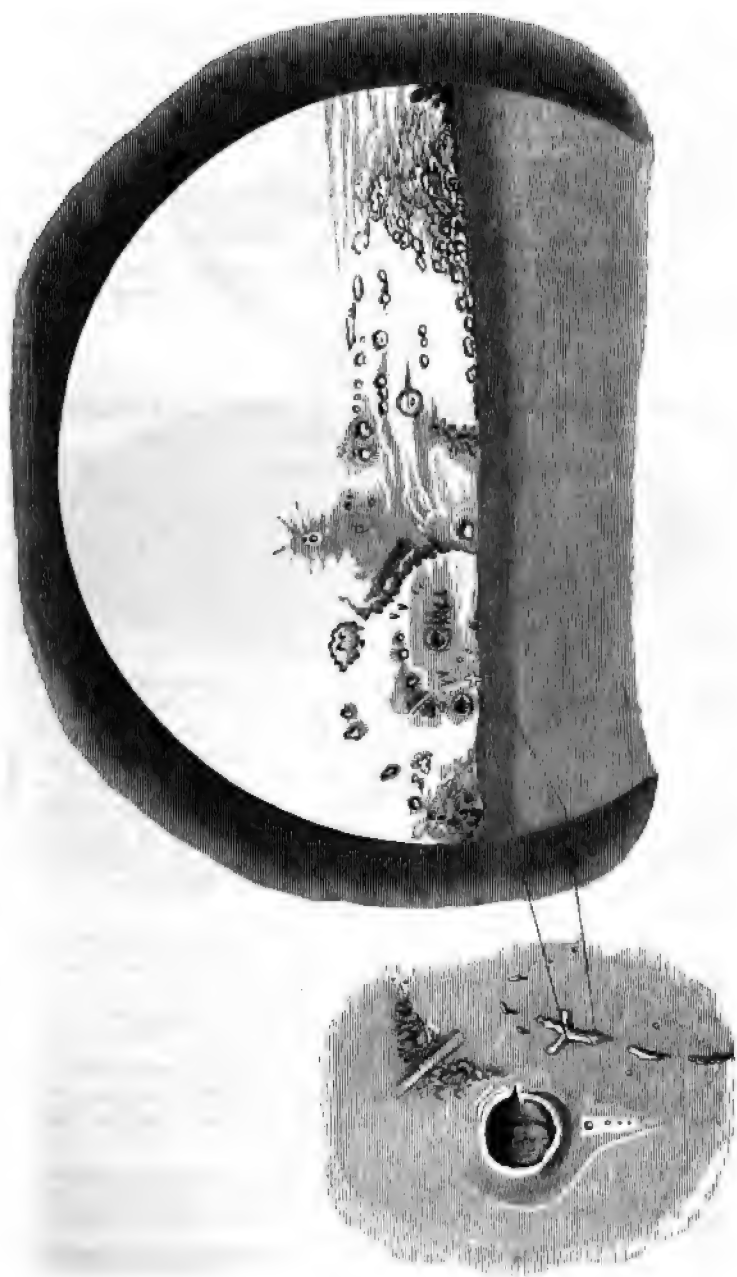
"I have made a sketch, which I enclose, of the region where they were; and again when she was two days older, and when the ridge was full in the light I made the enlarged sketch.

"I have no doubt the light was from the two small mouths shown in the angles of the cruciform ridge.* . . . In looking at the ridge, as drawn, the form of the lights is just what we should expect anything flowing from these mouths would take. If they are mouths, which I am assuming them to be, they are very small.

"My telescope is a reflector of 12 feet focal length and 10 inches in diameter, of our own making. The speculum has not been polished these twelve years, and may not show so sharp as it did at first."

Description of an Apparatus for the Mechanical Imitation of Precession. By T. W. Burr, Esq.

"Being desirous, for educational purposes, of obtaining some piece of mechanism which should represent the motion of precession in a more tangible form than can be done by diagrams on a plane surface, my attention, while considering the subject, was arrested by a description in vol. i. page 43, of the *Monthly Notices*, of an instrument invented by Mr. Atkinson, to illustrate certain phenomena of rotation. The account is very short and not very lucid, stating that 'it consists of a flat circular disk, through the centre of which passes a small endless screw, having a cup in one of its extremities.' When this cup is set on a point, and the disk made to rotate in its own plane, the whole rotating plane performs at the same time a slow revolution round the perpendicular to the horizon which passes through the point of suspension, and the direction of this revolution will either coincide with, or be the reverse of, the rotation, accordingly, as the centre of gravity is above or below the point of suspension; affording in the latter case, says Mr. Atkinson, 'an apt illustration of precession.'

"Acting upon this suggestion, and adding such improvements as enable the instrument more effectually to represent the real state of the case, I have had the apparatus submitted to the meeting constructed.† It consists of a small terrestrial globe, having a steel axis projecting from the north pole, and the equator extended into a broad brass disk. The globe is suspended on a point, rising into its interior from a base which supports a horizontal graduated circle representing the ecliptic; and the whole

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* The writer here makes a remark which is not very clear respecting the form of the luminous spots. The light on the right side would appear to have consisted of two streaks of unequal length disposed in opposite directions.— EDITOR.

† It was beautifully made by Mr. Hislop, chronometer-maker, 108 St. John Street Road.
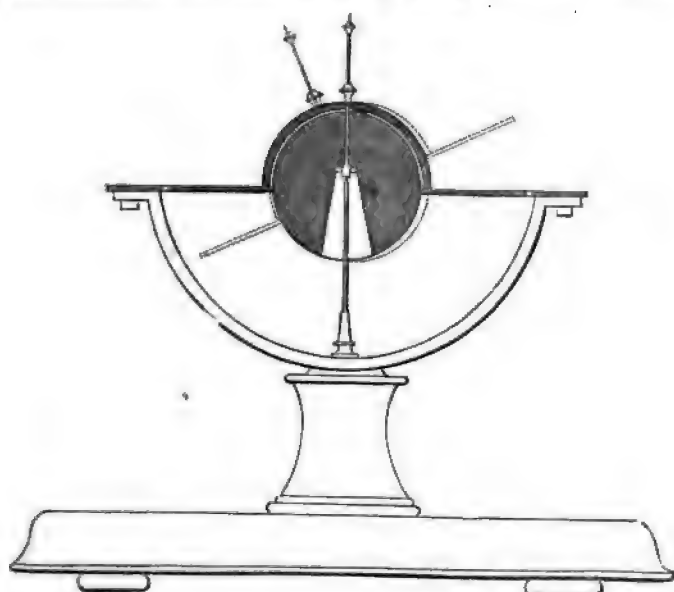
being nicely balanced, the ecliptic and equator coincide. If now a small weight be fixed to the equatoreal disk, to represent the attraction of the sun and moon, or the axis be considerably inclined, which answers the same purpose, and the globe be rapidly rotated, representing the diurnal motion, it will be seen that at the same time there is generated a slow conical movement of the projecting axis round the imaginary pole of the ecliptic, in a direction opposite to that of the diurnal rotation; and that the equinoctial points, or intersections of the ecliptic and equator, move round in the same way."

The author remarks that, upon attentive observation, it will be found that the axis does not describe a perfect circle round the perpendicular to the ecliptic, but that there is a minute undulation resembling the nutation of the earth's axis.

"I am aware that the mechanical causes of the motions set up are not the same with the physical causes of the real motions of the earth, but the object was to obtain an imitation of the effect, regardless of the exact identity of mechanical causes. So far as I understand the description of Mr. Atkinson's instrument, there is, of course, an identity of principle and effect, but he seems to have had other points to illustrate, and only mentions precession as an incidental one; whereas, in my construction of the apparatus, those additions are made which enable it to carry out the imitation to the fullest extent; and having myself found it useful in explaining the motions referred to, I have thought a knowledge of it might be acceptable for a like purpose to other members of the Society. The drawings show the apparatus in perspective and section, and are sufficient, with the preceding description, to explain it without references." *

Note by the Editor.

In the *Edinburgh New Philosophical Journal* for April 1855 there is a paper by Mr. James Elliot, Teacher of Mathematics, Edinburgh, which also contains a description of an apparatus

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* Apparatuses for exhibiting the conical movement of the earth's axis, which occasions the precession of the equinoxes, have also been contrived by Bohnenberger, Fessel, and Professor Powell. They are all unexceptionable, both in principle and practice, and serve to represent the phenomenon with the most complete fidelity. This remark will appear superfluous to many persons, but it has been deemed necessary to advert to the circumstance in consequence of objections having been urged against those apparatuses, which could only arise from an imperfect knowledge of the mechanical principles of their construction. Nor can such objections be justified by referring them to an imperfect working of the apparatus in any case, since from the nature of the mechanism it is impossible that the axis of rotation, if it have a movement at all, should have any other than a conical movement. In all these contrivances (as in Atkinson's original apparatus, now in the possession of Mr. Riddle at Greenwich) there is an arrangement of great simplicity, by which the conical motion of the axis of rotation may be rendered *direct*, as exemplified by the spinning-top or *retrograde*, as in the case of the earth's motion, thus affording a most instructive view of the mechanical principles which determine the grand phenomenon of the precession of the equinoxes.—EDITOR.

exactly the same as Atkinson's; but the author does not seem to be aware of the fact that any person had preceded him in the invention. The paper, which is entitled "A Description of certain Mechanical Illustrations of the Planetary Motion, accompanied by Theoretical Investigations relating to them, and in particular a new Explanation of the Stability of Equilibrium of *Saturn's* Rings," was read before the Royal Scottish Society of Arts on the 27th of February and 13th of March, 1854.* It contains several ingenious illustrations of the planetary movements in addition to that of precession. The author states that he was first led to consider the possibility of imitating the conical motion of the earth's axis by observing the whirling of a penny, and that afterwards, from reading Herschel's *Astronomy,* his attention was turned to the analogous movements of the te-to-tum and the top. "I observed, however," says he, "that in tops which have short pegs, this motion—the conical motion of the axis—is slower than in those which have long ones; and, in fact, the shorter the peg the slower the revolution. It therefore occurred to me, that if we could lower the centre of gravity till it coincided with the centre of motion, this movement would cease altogether, and the top would continue to spin with its axis pointing permanently in any direction in which it might be placed. I also concluded, if we still further extended the same change which gradually annihilated the positive motion, it would reappear negative or in the opposite direction. With that view I had an instrument constructed of the form showed in the annexed cut,† consisting of a wooden ball hollowed out in its lower part, so as to admit the support upon which it rests to be raised above the centre of gravity, and with a screw upon its peg or axis, to admit of its being raised or lowered at pleasure. I also confined it to one place, by forming a small cavity on the support for the point of the peg to run in. This being done, I was much pleased to find my expectations exactly realised. By adjustments of the screw the conical revolution could be quickened, retarded, annihilated, or reversed, as might be desired, and all its motions were brought under perfect control. At the same time it was surrounded by a fixed plane to represent the ecliptic, its own equator being marked upon it; and by forming the axis of hard steel and giving it a support of agate, its velocity could be kept up without much abatement for a long time.‡

"The rotation is produced in the ball by means of a string and handle, much in the same way as that in which a humming-top is spun. The case in which, from the two centres coinciding, the axis remains fixed in one direction without any conical revolution,

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* It is stated that the silver medal of the Society, value ten sovereigns, was awarded to the author on account of his paper.

† The drawing here alluded to is almost identical with that annexed to the preceding paper.

‡ The author here states in a note, that since the model described was constructed, his attention had been directed to Bohnenberger's instrument, of which he was not previously aware.

enables us to illustrate clearly what is meant in astronomy by the parallelism of the earth's axis, since the model may be carried by the hand slowly round in any circular or elliptic orbit, without any perceptible deviation of the axis from its original direction. But when the centre of gravity is brought slightly below the point of support, we are then enabled to show the deviation from parallelism which arises in the direction of the earth's axis after a long period of years, the same motion exhibiting the precession of the equinoxes. With the centre of gravity so placed, if the ball is made to rotate in the direction marked by the upper arrow on the figure, or from west to east, the equinoctial point E is observed to move slowly in the direction marked by the lower arrow from east to west. The latter motion may be made as slow as we please, so as to approach within any degree of closeness the exceedingly slow precessional movement of the earth's equator."

In another part of his paper the author makes the following remark : —

" If we next load the sphere on one side *very slightly* by any means, we obtain an illustration of the nutation of the earth's axis, the axis making a multitude of minute conical revolutions round the circumference of the greater conical revolution."

Note on the Method of Computing the Moon's Parallax.
By Mr. Sang.

In deducing the true from the apparent place of a heavenly body, it is usual to correct the observed altitude for the effect of atmospheric refraction, and to use the altitude thus corrected as the argument for the parallax; the sine of the parallax being supposed to be proportional to the sine of the corrected zenith distance.

This operation is founded on the hypothesis, that on correcting the observed altitude for refraction we obtain that altitude at which the planet would have been seen from the observatory if there had been no atmosphere. This hypothesis, however, is incorrect, as is evident when we consider that the ray of light, impinging on the upper surface of the atmosphere, is gradually bent downwards, and reaches the eye after having traced a curved path through the air. The angle of refraction is that angle which the first direction of the light makes with its direction when just entering the telescope; and, therefore, on correcting the observed altitude for refraction we obtain the direction, not of the original unrefracted ray, but of a line drawn parallel to it through the place of observation, which line cannot pass truly through the planet.

Thus if A be the observatory on the earth's surface, M the position of a celestial body, and M B the ray of light entering the atmosphere at B and then describing the curved line B A, the

line A D drawn to touch that curve at A gives the apparent position of the planet; and if we continue M B to meet the tangent in C, M C D is the angle of refraction. When, therefore, we add the refraction D C M to the observed zenith distance Z A D we obtain the zenith distance of a line drawn through A parallel to C M; and this corrected zenith distance exceeds that which the planet would have had if there had been no atmosphere by the angle A M B, under which the curved path B A would be seen from the planet.

In the case of the primary planets this angle A M B is much too small to be taken into consideration, but in the case of the earth's satellite it may possibly amount to something appreciable. My object in the present paper is to estimate the amount of error caused by neglecting this angle.

At first it would seem that for this purpose we must know the nature of the curve B A; and that thus all the uncertainties which perplex us in regard to the gradual attenuation of the air would complicate our inquiry. But, fortunately, it turns out that these considerations are eliminated, and that the strict computation of the parallax is more simple and more direct than the ordinary approximate computations.

Having joined M with E, the centre of the earth, E M B is the true parallax. Continue M B and D A to meet perpendiculars let fall upon them from E. Then it is a well-known property of rays refracted by concentric strata that the ratio of these perpendiculars E K and E L is that of the indices of refraction of the strata at B and A inversely.
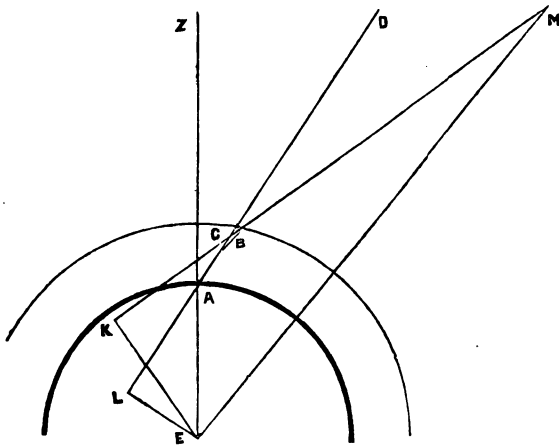
Now the index of refraction of the air at A can be obtained by direct experiment, or can be computed from the states of the barometer and thermometer, so that the ratio of E K to E L is known. Let this ratio be denoted by i.

Then we have

$$\sin EMK = \frac{EK}{EM} = i\frac{EL}{EM} = i\frac{EA}{EM}\sin ZAD.$$

But $\frac{EA}{EM}$ is the sine of the moon's horizontal parallax, on the supposition of there being no atmosphere; and, therefore, it appears that the sine of the actual parallax is obtained by multiplying the sine of the horizontal parallax by the sine of the observed zenith distance and by the index of refraction of the air.

The true argument for parallax is thus the observed, and not the corrected, altitude, the formula being

$$\sin par. = i \cdot \sin Hor. Par. \cdot \sin app. Zen. Dist.$$

On comparing this formula with the usual one, we find that the coefficient *sin corrected Zen. Dist.* has been replaced by *i . sin observed Zen. Dist.;* these two coefficients would be identical if the strata of the atmosphere were plane instead of spherical, so that the true computation must give a result differing more and more from the usual one as the angle A E B becomes larger, and we may expect the greatest deviation to be when the moon is in the horizon. When the apparent zenith distance is 90° the corrected is about 90° $33'$, and the sine of the corrected zenith distance, instead of being greater, is actually less than that of the apparent. For the purpose of roughly estimating the inaccuracy of the common formula we may take these sines as equal to each other; now the index of refraction of air at its mean density is $\frac{3405}{3404}$ nearly; and, therefore, for planets apparently on the horizon, the parallax exceeds that quantity which is given in the almanacs as Hor. Par. by $\frac{1}{3404}$th part of itself; which gives in the case of the moon at its mean distance almost exactly one second of arc.

The strict computation of the parallax would be at once effected, if, instead of the horizontal parallax, as given in the almanac, which is half of the angle under which the earth would be seen from the moon were there no atmosphere, we had the true horizontal parallax, or that angle which the earth, as magnified through the atmosphere, actually does subtend at the moon. The sine of this augmented horizontal parallax would then have to be multiplied by the sine of the observed zenith distance. It is true that such a table would serve strictly only for observations made in the mean state of the air; but the variations in the index of refraction scarcely affect the result; a change of one inch in the barometer only producing a change of $0''\cdot03$ in the horizontal parallax. With the present arrangement we can easily obtain the strict result by adding 1285 to the seven place logarithm of the parallax.

From this investigation it appears that, in reducing observations made on the occultation of fixed stars by the moon, it is scarcely necessary to take the actual amount of refraction into account at all: at most, in observations on moon-culminating stars, the differential refraction is required.

Waarnemingen gedaan te Manado ter bepaling van de Geographische lengte dier plaats, door S. H. De Lange en G. A. De Lange, Geografische Ingenieurs.

(*Communicated by the Astronomer Royal.*)

In the *Monthly Notices* for December last a brief account was given of a series of astronomical operations by the author of this paper, S. H. De Lange, in conjunction with his colleague, G. A. De Lange, having for their object the determination of the longitude of Batavia, chiefly by observations of the zenith distance of the moon. The most important feature of these operations consisted in the mode in which the zenith distance of the moon was determined. This was effected by first observing the moon, and then turning round the telescope in azimuth without unclamping it, so as to bring the star of comparison into the field of view. The operations for determining the longitude of Manado appear from the paper communicated on the present occasion to be precisely similar to those employed in the case of Batavia. The author, however, remarks, that the individual results are not so consistent in the present case,—a circumstance which he is inclined to attribute to the exposed situation of the telescope, and its consequent liability to be shaken by high winds. In reducing the observations of the moon's zenith distance, Adams's correction of the moon's parallax was employed. The observations extend from May 28, 1852, to January 21, 1853. As in the former paper, no definitive value of the longitude is given.

ERRATUM.

Page 49, line 2 from top, *for* Lieut. G. M. Gilliss, *read* Lieut. J. M. Gilliss.

CONTENTS.

M. J. Johnson, Esq. President, in the Chair.

Rev. Wm. Selwyn, Ely, and
W. R. Vines, Esq., Ealing, Herts,

were balloted for and duly elected Fellows of the Society.

Discovery of a New Planet. By M. Chacornac.

Admiral Smyth has received a letter from M. Chacornac announcing his discovery of a new telescopic planet of the eleventh magnitude at the Imperial Observatory, Paris, on the 6th of April. The following positions of it were obtained by a comparison with the star 25438 in the Catalogue of Lalande:—

1855.	Paris M.T. h m s	Right Ascension. h m s	Declination. o ′ ″
April 6	13 10 49·6	13 39 50·02	−7 28 7·8
	28 5·6		
	15 32 53·9	13 39 45·19	−7 27 24·2
	51 40·1		

This discovery was also announced in a printed circular issued by M. Le Verrier, Director of the Imperial Observatory.

Elements of Chacornac's Planet. By M. Lesser.

Epoch, 1855 April 20·45093 M.T. Berlin.

$$
\begin{aligned}
M &\dots\dots\quad 38\ 48\ 53\cdot0 \quad & \\
\varpi &\dots\dots\quad 157\ 51\ 19\cdot7 \ \big\} & \text{Mean equinox,} \\
\Omega &\dots\dots\quad 184\ 1\ 45\cdot3 \ \big\} & 1855\cdot0 \\
\varphi &\dots\dots\quad 6\ 34\ 5\cdot0 & \\
i &\dots\dots\quad 5\ 10\ 10\cdot5 & \\
\log a &\dots\dots\quad 0\cdot426961 & \\
\mu &\dots\dots\quad 2\cdot909565 &
\end{aligned}
$$

These elements are calculated from the following observations:—

1855.	Berlin M.T. h m s	R.A. o ′ ″	Decl. o ′ ″	
April 6	14 44 14·0	204 57 5·3	−7 27 58·1	Paris
13	10 2 54·1	203 36 35·4	−6 38 57·2	Vienna & Berlin
20	10 49 20·7	202 12 53·3	−5 49 36·6	Berlin

Discovery of a New Planet. By M. Luther.

Mr. Hind has received a letter from M. Luther, announcing his discovery of a new planet of the eleventh magnitude on the 19th of April, at the Observatory of Bilk. The following are two of its positions as provisionally reduced:—

	Bilk M.T.	R.A.	Decl.
1855.	h m	o ′	o ′
April 19	13 30	181 14	−5 11
20	10 27	181 6	−5 11
	Daily motion −9		0

Discovery of a New Comet (Comet II., 1855).
By M. Schweizer.

On the 11th of April, about 11 o'clock in the evening, Dr. Schweizer, Astronomer, attached to the Geodesical Observatory of Constantine at Moscow, discovered a small telescopic comet, for which he obtained the following approximate positions by reference to Argelander's Star Charts:—

	M.T.	Comet's R.A.	Comet's Decl.
1855.		o ′	o ′
April 11	11	184 40	−17 20
15	11	182 20	−13 40

Its daily motion is accordingly at present about,—

$$\text{In R.A.} \quad \ldots\ldots\ldots\ldots \quad -35' \ 0$$
$$\text{In Decl.} \quad \ldots\ldots\ldots\ldots \quad +55 \ 0$$

On the Method of Observing the Positions of Spots on the Sun, and of Deducing their Heliographical Longitude and Latitude, adopted at Redhill Observatory. By R. C. Carrington, Esq.

" No change has been made in the mode of observation described in No. 5 of vol. xiv. of the *Monthly Notices*, but an improvement of the process of reduction has been introduced, which it is the object of this second communication to explain.

" Referring generally to the former description, and as before denoting by the letters A_1, A_2, B_1, B_2, the instants of contact of the sun's limb with the wires $A a$, $B b$, and by the letters a, b, those of a spot,

let $\qquad A = \frac{1}{2}(A_1 + A_2), \quad B = \frac{1}{2}(B_1 + B_2)$

also let $\quad \tan a_1 = \dfrac{B_2 - B_1}{A_2 - A_1}$, and $\tan a_2 = \dfrac{b - B}{a - A} \cdot \cotan a_1$,

then will $\alpha_1 + \alpha_2 + \iota$ be the angle of position of the spot on the disk from the north point reckoned towards the east, ι being the correction due to the inclination of the sun's path to a parallel of declination, and very nearly equal to four times the hourly increment of declination given in the *Nautical Almanac*, with its proper sign. If we denote the distance of the spot from the centre of the disk, expressed as a decimal of the radius, by $\frac{r}{R}$, we shall have

$$\frac{r}{R} = 2 \cdot \frac{b-B}{B_2-B_1} \cdot \operatorname{cosec} \alpha_2, \text{ or } = 2 \cdot \frac{a-A}{A_2-A_1} \cdot \sec \alpha_2$$

the first or the second expression of the two being used, accordingly as $b - B$ is greater or less than $a - A$.

"Let (R) be the sun's tabular semidiameter expressed in minutes of arc, and let $\zeta' = \frac{r}{R} \cdot (R)$, then will ζ, the angular distance of the spot on the sun's body from its apparent centre, be found from the expression $\zeta = \sin^{-1} \cdot \frac{r}{R} - \zeta'$, the angle $\zeta + \zeta'$ being taken out to minutes and tenths.

"The quantities $\alpha_1 + \alpha_2 + \iota$ and $\frac{r}{R}$ give the means of forming a diagram of the appearance of the disk.

"*Second Step.*—Let the inclination of the sun's equator to the ecliptic be denoted by I, and the longitude of its ascending node by N. (The provisional values used at Redhill for 1854·0 are I = 7° 10′, and N = 74° 30′.) It is convenient to tabulate, for every degree of argument, the angles

$$\beta = \tan^{-1} (\cos \odot \cdot \tan \omega), \qquad \gamma = \tan^{-1} (\cos \overline{\odot - N} \cdot \tan I),$$

$$\alpha = \tan^{-1} (\tan \overline{\odot - N} \cdot \cos I), \quad \delta = \cos^{-1} (\sin \overline{\odot - N} \cdot \sin I),$$

where α and δ are respectively the heliographical longitude and north polar distance of the earth.

Let
$$\chi = \overline{\alpha_1 + \alpha_2 + \iota} + \overline{\beta + \gamma}$$
$$\cos \varphi = \cos \zeta \cdot \cos \delta + \cos \chi \cdot \sin \zeta \cdot \sin \delta,$$
$$\sin \theta = \sin \chi \cdot \sin \zeta \cdot \operatorname{cosec} \varphi$$

then will $\overline{\alpha - \theta}$ and φ be the heliographical longitude and north polar distance of the spot.

"It will be remarked that no account is here taken of the effect of refraction. The reason is, that a little experience convinced me that, unless its effect could be allowed for in current reductions in some very simple manner, it was better to omit it; and I have not been able to devise any mode of computing a correction for it at all corresponding in brevity to the above formulæ.

"The brevity of the formulæ now given will best be shown by an example, and for this purpose we will take an observation of the principal nucleus of the large spot seen in January last.

" The mean of three passages on Jan. 18th gave the following numbers :—

A$_1$	19 36 28·867	Slow 55^s·5	Longitude 41^s·3 W.
B$_1$	36 43·867	G. M. T. of obs.	1855, Jan. 17^d 23^h 51^m.
a	37 40·133	$\odot$ = 297 53·7	β = + 11 28·8
b	38 5·067	N = . 74 30·7	γ = − 5 13·2
A$_2$	39 48·067	$\odot$ −N = 223 23·0	ι = + 2·1
B$_2$	19 39 59·800	α = 43 9·5	δ = .94 55·0

B$_2$−B$_1$ = 195·933   Log = 2·29211   Log tan α_1 = 9·99282
A$_2$−A$_1$ = 199·200   Log = 2·29929 α_1 = 44° 31'·6

b − B = − 16·767   Log (b − B) = −1·22445
a − A = − 28·334   Log (a − A) = −1·45231
Log (a − A) = 1·45231   Diff. = + 9·77214
Log (A$_2$ − A$_1$) = 2·29929   Log tan α_1 = 9·99282
Diff. = 9·15302   Log tan a_2 = + 9·77932
Log sec α_2 = 0·06708 α_2 = 211° 1'·9
Log 2 = 0·30103 α_1 = 44 31 ·6
Log $\frac{r}{R}$ = 9·52113 ι = + 2 ·1
Log (R) = 1·212 $\beta + \gamma$ = + 6 15 ·6
Log ℓ' = 0·733 χ = 261 51 ·2

Log cos ϱ = 9·97488 $\varrho + \ell'$ = 19 23 ·4

Log cos δ = −8·93301 ℓ' = 5 ·4
Log n_1 = −8·90789 ϱ = 19 18 ·0
Log cos χ = −9·15140   Log sin χ = −9·99559
Log sin ϱ = 9·51919   Log sin ϱ = 9·51919
Log sin δ = 9·99840   Log cosec φ = 0·00357
Log n_2 = −8·66899   Log sin ℓ = −9·51835
n_1 = −0·08089 ℓ = −19° 15'·6
n_2 = −0·04667 α = 43 9·5
$n_1 + n_2$ = −0·12756 $\alpha − \ell$ = 62 25
Log ($n_1 + n_2$) = −9·10571 φ = 97 20

" The result of the calculation is accordingly,—

Angle of position, 255° 35''·6   Dist. from centre, 0·3320
Heliographical long. 62° 25'   Latitude, 7° 20' South

" As was mentioned in the Annual Report lately published, this process of reduction has been applied at Redhill to all the observations of spots which were obtained during the past year, and a tolerably continuous catalogue of positions and of corresponding diagrams is consequently accumulating. As I feel well satisfied after a full trial with the method now put forth, and think it improbable that a shorter one can be devised, I

venture again to express a hope that those who interest themselves in this subject will give my method a trial.

"The following series of observations of a small spot may be useful as exhibiting the degree of accuracy of the results obtained:—

1854.	G.M.T. h m	Pos. ° '	Dist.	H. Long. ° '	H. Lat. ° '
Aug. 25	0 12	123 2	·9399	189 24	S. 10 14
26	0 51	126 44	·8445	203 23	10 20
27	1 32	132 9	·7022	218 10	10 3
28	0 41	141 24	·5481	231 56	10 13
29	0 38	159 58	·3912	246 19	10 23
30	0 32	195 9	·3031	260 29	10 23
31	1 6	235 23	·3611	275 6·	10 19
Sept. 1	0 24	256 34	·5033	288 54	10 8

4 The spot had entirely disappeared.

"Such series, it is needless to state, are suitable for a basis for the further correction of the assumed elements of the position of the sun's poles. I have, of course, several such series already observed, but they are not yet sufficiently numerous to warrant the adoption of any certain corrections of the assumed values of I and N. The problem of correcting the assumed position of the sun's axis from such data is similar to the case of finding the error of position of the polar axis of an equatoreal from three or more observations in north polar distance of the same star at different hour-angles.

"My observations during 1854 suggest one or two remarks which may be appended here, although they are, I hope, by no means all that they will ultimately afford.

"1. The year 1854 has been one in which the number of outbreaks has been very few,—indeed, approaching a minimum. Now the reduced list of positions includes no spot more distant from the equator than 19° 30' to the north, and 17° 0' to the south. This circumstance appears to afford grounds for concluding that when the eruptive forces are comparatively quiet, the limits of parallel are also much contracted.

"2. On the 29th of September I observed a moderate-sized spot in position 110° 17' from the north point, distance from centre = 0·9323, the penumbra of which included two detached nuclei; on the following day, the 30th, it was observed that the 'following' nucleus was outside the penumbra. Careful diagrams were made at the time on both days. I cannot reconcile this observation with the commonly received idea of the relation of nucleus and penumbra. As an isolated instance, it does not appear to call for more than being recorded at present; or rather I may say that, until other instances shall be met with, it appears to me to be premature to state the inferences which the observation seems to suggest.

"3. Throughout the year 1854 there has been a continual

tendency to recurrence of spots on the same parallels of latitude, both of individual spots on almost precisely the same parallel, and of groups within the same general limits. The parallels 71° to 75° Heliog. N.P.D. have been but rarely visited; the parallels 77° to 85°, frequently; from 86° 30' to 89° 30', not at all; from 90° to 92°, very rarely; from 92° to 94°, not at all; from 95° to 103°, frequently; from 103° to 107°, very rarely. In connexion with this circumstance, I would suggest whether it be not worth while to examine into the possibility of an explanation arising from the hypothetical revolution of an inner body, on which may be situated the volcanic centres of eruption, in a shorter period than the revolution of the envelope on which we see the effects in the form of ruptures of continuity. It will appear to any one who considers this suggestion that, to bring it to the test of facts, there will be required a collection of observed positions of nuclei on the same parallel, together with the times of their first appearance on the envelope; and accordingly, I would recommend fellow-labourers on this subject to be careful to record the occurrence of outbreaks which they may succeed in ascertaining to have taken place between two consecutive days."

Remarks on the Orbits of α Centauri and σ Coronæ Borealis.
By Capt. W. S. Jacob, Astronomer at Madras.

In the *Monthly Notices* for January are given the elements of two orbits for α *Centauri*, contributed by Messrs. E. B. Powell and J. R. Hind. The latter, probably, represents the observations pretty fairly, but the former can scarcely be said to do so, as it makes the observed distances before 1846 all too great, and after that epoch all too small, while even in the angles there is in a general way the same opposition of signs, though not quite without exception; in such a case, it is evident that the sum of the errors or of their squares must be capable of considerable reduction.

" But, in fact, the agreement of the orbit with observation is no proof that it is even an approximation to the true one: where we have given only a small portion of the periphery of an ellipse, it is evident that with a very slight change of curvature we may adapt it to many different ellipses; and, even when we are pretty sure of the dimensions of the apparent ellipse, it may happen that a very slight change of position of the projected focus may cause a great variation in the true elements. This is the case in the present instance: I have computed a number of orbits for this star, but for some years have given up the attempt, as the data are insufficient; though we can lay down the apparent orbit very closely, we cannot as yet get even a decent approximation to the true one.

" The reason is this: La Caille's observations in 1751, though not, of course, minutely accurate, enable us to fix the periodic time within a small quantity; since the stars had returned to the same

relative position about 1830, the period must evidently be about 79 years, within a year or two either way ; and as we know pretty exactly the area now described annually by the radius vector, we can also fix nearly the total area of the apparent ellipse. The path described from 1834 to 1854, the only time within which trustworthy observations are available, is shown on the slip marked X, and is of such extent and small curvature that it must
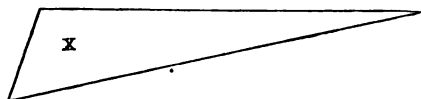
include the extremity of the minor axis, and having given the curvature at this point and the area, the dimensions of the ellipse are fixed. It appears that the axes must be about 28″·80 and 6″·86, as shown in the lower diagram. If we now try to fit the slip X to the periphery of the ellipse, we find it may take the position A B C ; but, by reason of the slight change of curvature
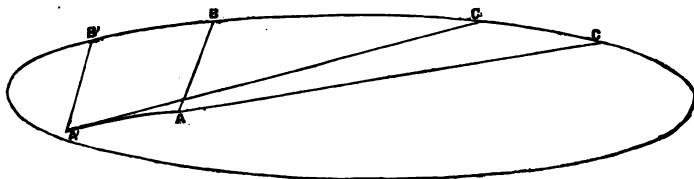
in so elongated an ellipse for a considerable distance on either side of the minor axis, it may also be *slided* round the curve as far as A′B′C′ without material error, the locus of the principal star, or projected focus, being the thick line A A′, and, with the exception of not being *very* likely to fall quite close to either extremity, it may as well occupy one part of the line as another, and the observations will be equally well represented whatever point be fixed on. This, then, allows a range in per. pass. of from 1858·5 to 1867·8, and in *e* from ·54 to ·96 ; while *a* may range from 15″ to 31″. These limits may be a little further extended by slight variations in the dimensions of the apparent ellipse, but they will be somewhat narrowed by the end of the current year; for if the first position be nearly true, the places at 1856·0 will be 309°·0 and 3″·75, and if the second, 312°·6 and 3″·45,—a difference which observation would be sure to detect ; still we shall not be able to get a good approximation until the extremity of the ellipse has been reached, or nearly so.

" σ Coronæ Borealis.

" In the same Number (page 90) is given, also by Mr. Powell, an orbit for σ *Coronæ Borealis*, which appears from the comparisons cited to agree closely with observation. There are, however, a few epochs omitted where the agreement would not be

quite so close; it may also be doubted whether the plan of grouping together several epochs into one is quite legitimate, for in the case of such quick-moving stars the mean of angles extending over two years would not correspond to the mean of the times.

" The following elements will be found to represent the observations pretty fairly, but they will admit of considerable variation :—

$$\tau = 1831\cdot17$$
$$\varpi = 107°\ 13'$$
$$\Omega = 1\ 57$$
$$\lambda = 101\ 57$$
$$\gamma = 46\ 47 \quad \cos = [9\cdot83552]$$
$$e = \cdot3088$$
$$P = 195^{yr}\cdot12 \quad n = 1°\cdot845$$
$$a = 2''\cdot717$$

Comparison.

Date.	Observer.	ℓ_o		$\dfrac{\ell_c - \ell_o}{\text{arc.}}$	ϱ_o	$\varrho_c - \varrho_o$
		° '		' ''	''	''
1781·79	♅	347 32	− 46	·038		
1802·74	,,	11 24	+ 85	·058		
1819·60	Σ	50 18	+183	·081		
1821·30	H & S	65 15	−302	·129		
1822·67	Σ	61 0	+300	·123		
1823·47	H & S	72 56	−199	·082	1·45	−·055
1825·44	S	77 31	+104	·040	1·48	−·160
1826·77	Σ	89 0	−171	·065	1·30	+·012
1828·50	H	92 6	+183	·069		
1830·28	H	105 5	− 36	·014	1·22	+·092
1830·52	D	107 17	− 93	·035		
1830·76	Sm	107 36	− 37	·014	1·30	+·014
1831·34	D	111 32	− 93	·035	1·57	−·250
1831·36	H	108 46	+ 79	·030	1·39	−·070
1832·37	Sm	114 54	+ 21	·008	1·40	−·054
1832·55	D	115 57	+ 13	·005		
1833·36	,,	120 37	− 43	·017	1·30	+·084
1833·58	Sm	120 42	+ 14	·006	1·20	+·190
1835·50	,,	130 54	− 86	·033	1·40	+·048
1839·67	,,	145 6	− 6	·003	1·60	+·064
1843·35	,,	155 54	− 2	·001	1·80	+·015
1846·21	j	161 58	+ 49	·027	2·25	−·300*
1853·14	,,	177 54	− 54	·034	2·18	+·040
1853·35	Powell	175 12	+130	·083		
1854·05	j	177 52	+ 45	·029	2·22	+·036

* Only a single night's measure, and not very good.

" N.B. As it is desirable that the same observer should be always designated by the same letter, and as J has been already appropriated by Manuel Johnson, it will be better to keep to *j* for designating my observations."

5 March, 1855.

Description of an Observatory erected at Grantham.
By J. W. Jeans, Esq.

" I erected this observatory about five years since. The transit-room is placed between the two roofs; the timbers which carry the floor-joists being firmly spiked to the rafters of the roofs, abutting against their tye-beams; the pedestal of the transit is quite free from the floor; the walls are of wooden frame-work covered with sheet-iron, canvassed and papered inside; the roof ditto ditto, but having felt under the iron, which is galvanised. The equatoreal-room is constructed in a similar manner; the joists of the floor being carried by the outer or western purling of the roof, at one end, passing quite clear of the inner or eastern purling, and being carried at the other end by timber uprights passing down to the party wall. A large box full of sand is attached to the eastern purling, upon which the equatoreal rests, quite independent of the floor, and but little affected, except by the vibration of the house itself; the walls of this part are lined with boards; the roof consists of light wood framing, covered with felt and sheet iron; it has double shutters, about 22 inches wide, opening quite across from curb to curb, in four sections closed at top by a square shutter, which can be raised so as to allow observations quite in the zenith. The curb is made of American oak, top and bottom double, lined with one-eight inch sheet-iron, and running on cast-iron balls. I cannot state the cost of erection, as it was partly constructed of old materials, and I did a great deal of the work (nearly the whole of the dome) myself.

" The transit is by Cary, 2 ft. long, 2 in. aperture, 6 in. circle, divided to 30′, reads to 1″; striding level; it is very steady to its adjustments, and performs very well. The equatoreal also by Cary, $3\frac{7}{8}$ in. aperture, 5 ft. 9 in. solar focus, circles 9 in. diameter, H. C. divided to 1$^{\rm m}$ reads to 2$^{\rm s}$, D. C. divided to 15′ reads to 30″; it performs very fairly. The clock-work to it is of mine own adaptation. The clock is an old one, Grantham make, dead beat, and furnished with a wooden compensating pendulum-rod, of mine own construction; its rate is very steady and good. My position, according to mine own observation, is

Lat.   52 24 52 North
Long.   0 39   0 West.

[The foregoing description was accompanied by a complete and very neatly executed drawing of Mr. Jeans's observatory.—EDITOR.]

On the Theory of M. Foucault's Gyroscope Experiments.
        By the Rev. Baden Powell, M.A. F.R.S., &c.

The remarkable experiments of M. Foucault for exhibiting both a new direct proof of the earth's rotation, and the indirect effect of it in the " orientation " of the axis of a disk influenced only by mechanical rotation, have excited great interest; and thus an attempt to elucidate and simplify the *theory* of them may not be regarded as useless, especially as that *theory* has been found in some degree obscure and complex, more particularly as treated on the principle of M. Poinsot's theory of " couples." See *Comptes Rendus,* tom. xxxv.

The apparatus (or " gyroscope ") consists essentially of a circular metallic disk, heavily loaded round its circumference, rotating on an axis, whose ends turn with the most perfect freedom in the opposite points of a brass circle, itself supported at the intervening opposite points on pivots in a second brass circle, suspended in a vertical plane by a thread without torsion at the top, and resting on a pivot at the bottom, both attached to an outer vertical frame. The motion of either of these circles can be suppressed, so that the axis of the disk can move either in altitude or in azimuth only, or freely in both (see fig. 1).
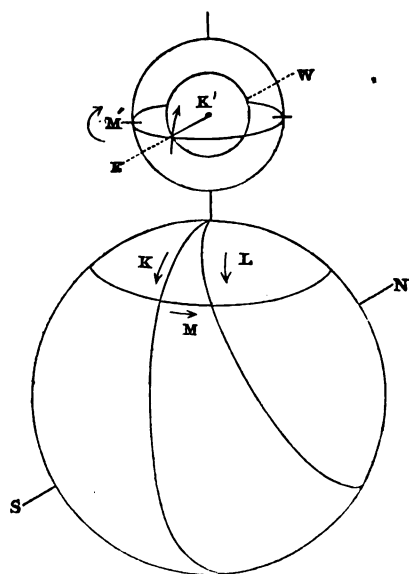
Fig. 1.

In these experiments the most material mechanical condition is what M. Foucault terms " the constancy of the plane of rotation,"—that is, the loaded disk having an immense rotatory

velocity communicated to it by the action of a train of wheels before it is placed on its pivots, retains, by virtue of its great acquired momentum, this motion *in the plane in which it was communicated*, independently of gravitation or any other circumstances,—so much so, as to resist with considerable force any attempt to place it in a different position.

In the following brief explanation it may be necessary to premise,—

1. The principle of the composition of rotatory motions, or that if a body be rotating about a given axis, and a tendency to rotate about another axis at the same time be given to it, the two rotations compounded will give a resultant rotation about a new axis inclined in a direction between the two former, whose position is determined by the well-known theorem, for which see Mr. Airy's tract on Precession.

2. The inverse principle of the resolution of rotatory motion, as applied by Euler to the rotation of the earth at any point on its surface, where the whole rotation being in a plane passing through the parallel of latitude of the place (which, for brevity, I call " the plane of latitude "), and the rotation for a given time (L) in that plane, it is resolved into two components, one in a circle passing through the place perpendicular to the meridian (K), the other in a circle (M) cutting the last at right angles, and whose centre is the place of observation. It is easily seen that the component (K) is *initially* proportional to the *cosine*, and (M) to the *sine* of the latitude (see fig. 1).

3. In any composition of forces p and q, of which r is the resultant, if the same action be repeated with p and r as components, the new resultant r_1 will lie between p and r. If it be again repeated r_2 will lie between p and r_1, and so on, till ultimately r_∞ will coincide with p.

Case I.—The disk rotates, being in perfect equilibrium, and free to move both in altitude and azimuth (see fig. 1).

Let us suppose the disk to have its axis in the first instance horizontal, and pointing east and west.

Then the rotation of the earth carries it along with the apparatus, and, if at rest, would cause it to incline towards the east in the plane of the latitude.

But, owing to " the constancy of the plane of rotation " the disk tends to retain parallelism to its original plane; and being perfectly free to move with respect to the apparatus, and wholly free from the influence of gravitation, it has a relative apparent motion equal and opposite to that of the earth in the plane of the latitude (L), the components of which in altitude (K') and in azimuth (M') are exhibited by the moveable circles of the apparatus respectively.

The motion in altitude would be inconvenient to observe, as it changes its plane continually; that in azimuth is easily observed, by means of a horizontal microscope with focal wires, directed to a small graduated scale on the edge of the vertical circle.

These motions continue with the rotation of the earth as long as the rotation of the disk is kept up: and the *rate* of the azimuthal motion is easily seen to be proportional to the *sine of the latitude* of the place, as in the case of the free pendulum.

Case II.—The disk rotates, having its axis horizontal, and free to move only in azimuth.

Let the plane of the disk (*d*) be at first parallel to the meridian (see fig. 2), or have its axis pointed E. and W.
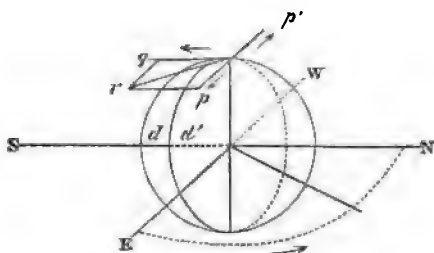
Fig. 2.

Then the constancy of the plane of rotation opposing the effect of the earth's rotation in the plane of the latitude tends to make the disk incline in that plane from east to west, initially in the direction (*p'*) of the tangent to the arc which its upper extremity would describe. But the vertical diameter of the disk being fixed by the apparatus relatively to the earth, offers an equal and opposite reaction (*p*).

This has to be compounded with the rotatory motion of the disk (*q*), (suppose towards the south, at the upper part), which will give a resultant rotation (*r*) in a plane between the two; or the disk will take a new position (*d'*), to which (*r*) is a tangent, in a plane inclined to that of the meridian, its axis turning in azimuth from east towards north.

In this new position the same action will be repeated, the components being now (*p*) and (*r*), and the new resultant (*r*₁) will place the axis between the former position and the north, and so on, till it ultimately points to the north, coinciding with the meridian, in which position it will continue.

If the rotation had been the opposite way it is easily seen; a similar construction would give the disk turning from north towards east. And in either case, when in the final position, the rotation will be in the same direction as that of the earth.

Case III.—The disk rotates, having its axis free to move *only* in altitude, and in the plane of the meridian.

In the first instance, suppose the axis horizontal, and the rotation towards the east at the upper part (see fig. 3).

The disk, if free, would have an apparent motion opposite to that of the earth, or the eastern extremity of its horizontal diameter

would have a motion upwards in the plane of the latitude, whose initial direction (p^1) is the tangent of the arc it would describe.
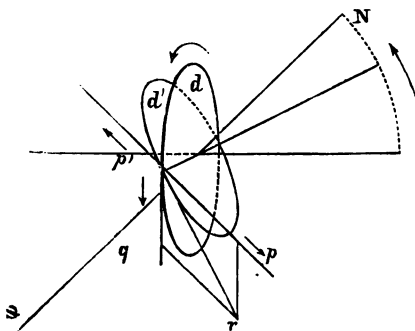
Fig. 3.

But this motion is counteracted by the apparatus giving an equal and opposite reaction downwards (p). This, combined with the rotation (q), gives a resultant (r) between them, in consequence of which the disk (d) takes a new inclination (d'), its axis ascending at the north end, till ultimately it becomes parallel to the axis of the earth, when the rotation of the disk will be in the same direction as that of the earth. If it had been originally rotating in the opposite direction, it is easily seen in the same way that it would come round to the same position.

* * *

Report of the Director of the Imperial Observatory of Paris on a Plan for Improving the Organisation of that Establishment.

In accordance with a decree of the Government, the Director. of the Imperial Observatory of Paris has recently drawn up a Report on a proposed plan of operations to be pursued at that establishment with the view of placing it on a level with the other Observatories of the first class in Europe. The details of the plan are embodied in a Report recently addressed to the Emperor by M. Fortoul, Secretary of State for the Department of Public Instruction. The contents of this Report, of which the following is the substance, are classed under seven distinct heads.

1. *Meridian Observations.* — Operations of this kind were long imperfectly attended to in France. The first transit instrument was erected at the Paris Observatory only in the year 1800. Since that time foreign establishments have accomplished from year to year considerable progress. In order that the Observatory of Paris may maintain itself on a level with them, it is indispensable that an improvement be effected in the instruments available for that purpose, as well in regard to optical power as in

regard to stability. It will be necessary, besides, to construct a large meridian circle, and to economise the resources presented by dynamical electricity, in order to assure to the observations all the precision of which they are susceptible.

2. *Extrameridional Observations.*— The Observatory of Paris not having yet taken a sufficient part in labours of this kind, it will be necessary to establish an entirely new branch of service, in order to respond to the important interests which are dependent on them.

3. *Comparison of Observations with Theory.*— Observations were formerly published in their rude state, without appending to them the labour of discussion which acquires for them a scientific value. It is not so in the present day: foreign establishments in Europe and America have followed the example given by England twenty-five years ago, and publish only discussed observations.

The Observatory of Paris is one of those which have not yet adopted this improvement. It will be the more difficult to realise it in practice, inasmuch as the ephemerides which have served as the basis of reduction are still in an imperfect state. A plan in reference to this object, based upon eminently scientific principles, is stated to have been proposed by the Director of the Imperial Observatory.

4. *Relations with the Public Service.* — It is extremely desirable that-the Observatory of Paris, after having made due provision for the advancement of science, should so dispose its resources as to enable the Government to profit by them. The study of the chronometers of the Imperial Marine might thus be improved, and the same advantage might be accorded to the Mercantile Marine. The different astronomical and meteorological instruments used at sea might also be compared at the Observatory.

The exact time might be sent daily by telegraph to the different parts of the empire, where it is to be obtained at present only imperfectly and for money. On extraordinary occasions it might be desirable to transmit to them atmospheric instructions, which might be necessary for the security of the shipping.

In order to determine with greater accuracy the principal data upon which the map of France has been constructed, it is suggested to unite the resources of the Observatory with those of the Ministry of War. It would be also desirable to transmit the exact time to the principal points of the Metropolis, &c. &c.

5. *Operations connected with Physical Science.*—The progress of astronomy is intimately allied to that of physics, and the latter tends to approximate towards astronomy as well by the services which it renders as those which it receives by borrowing its instruments and methods.

The establishment at the Observatory of a department of physical observations placed under the direction of a *savant* specially appointed for that purpose, is indispensable in the present day, not less for the wants of astronomy than for the solution of the great questions of modern physics. Such an establishment would

be extremely desirable for maintaining the optical instruments in a high state of efficiency, and for assisting artists in the fabrication of large glasses.

The use of telegraphic apparatuses, the employment of electricity in daily observations, as well as the relations of the observatory with the public service, necessarily demand the attention of a person well acquainted with experimental science.

It would also devolve on the physicist to investigate the advantages which might accrue to the sciences of astronomy and meteorology from the application of photography. Thus the photographic and instantaneous observation of the solar disk, by rendering available a source of light, the extreme intensity of which has hitherto been an object of dread, would assuredly furnish precious materials respecting the physical constitution of the sun.

The vertical has hitherto been considered as fixed, and yet theory indicates that gravity undergoes small variations both in intensity and direction under the influence of the combined attractions of the sun and moon. The investigation of apparatuses capable of indicating this kind of influence, and thereby furnishing new determinations of the masses of the sun and moon, would form an admirable subject of study to the physicist.

Finally, the velocity of light and of electricity has been established in the present day upon the surface of the earth. The same is true in respect to the rotation of the earth, a phenomenon of which the demonstration lately belonged exclusively to astronomy, but is now accessible to physics, which has furnished a proof of its existence that is sensible to all eyes. Still it may be affirmed that the labours undertaken with respect to this object, notwithstanding the just interest which they have excited, are yet only commenced. They might be resumed at the Observatory, receive there the last degree of precision, and thus be made to furnish important data for a knowledge of the solar system. This section concludes wlth recommending M. Leon Foucault to the office of Physicist to the Observatory.

6. *Meteorological Operations.* — It is also to the Physicist of the Observatory that the direction of the meteorological operations would belong, and this is not the least important part of the labours intrusted to him.

The establishment of model instruments, and of self-operating instruments; researches on the temperature of the air; on hygrometry, on pluviometry, on winds, on atmospheric electricity, on terrestrial magnetism, on atmospheric phenomena, such as halos, auroræ boreales, &c.: all this branch of meteorological observations is still to be created at the Observatory of Paris.

Even although confined to a single station, these studies would be of great utility. They would offer still greater interest if it were possible to extend them over the whole surface of the earth, and to trace across continents and seas the propagation of great atmospheric waves. It would be necessary for this purpose to

collect together the materials established at a great number of stations, and to institute a comparison between them. The electric telegraph promises in the present day to accomplish this important work with the most complete success.

7. *Scientific Administration.*—This section includes theoretical instruction, and also practical instruction in the art of observation, the care of publications and the charge of a vast and growing correspondence. In order to assure the improvement of these different departments it would be desirable to increase the salaries of the officials of the Observatory who are not remunerated in proportion to the difficulty and importance of their labours.

By an Imperial decree subsequently issued, the situation of Physicist of the Observatory has been instituted, and M. Leon Foucault has been appointed to this new office. The salaries of the astronomers connected with the Observatory have also been increased in pursuance of the same decree.

ERRATUM.

Page 161, line 3 from bottom, *for* agreement, *read* argument.

CONTENTS.

M. J. Johnson, Esq. President, in the Chair.

Rev. F. Silver, Norton, near Market Drayton, Shropshire, was balloted for and duly elected a Fellow of the Society.

On the Measured Distance of 70 Ophiuchi.
By the Rev. W. R. Dawes.

"The fine double-star 70 *Ophiuchi*, being quite within the optical power of comparatively small telescopes, has not received from me that constant attention which, perhaps, from its great intrinsic interest it deserves. As, however, the elements of the orbit recently computed by Mr. E. B. Powell give a continued *increase* of distance, while the measures of several observers show that that element is *decreasing*, I am induced to anticipate in this instance the publication of the results I have obtained, forming a portion of a large mass of observations, which, I hope, will before long be in the hands of the Astronomical Society.

"My mean results are the following :—

Pos.	Obs.	Nights.	Dist.	Obs.	Nights.	Epoch.
118·83	15	3	6·804	16	3	1848·12
114·66	29	7				1853·60
			6·489	24	5	·68
113·71	18	4	6·339	14	3	1854·73

"These measures fully support the conclusion indicated by those of Captain Jacob, Mr. Miller, and Mr. Fletcher; and, indeed, do not differ widely from any of them. The decrease of distance to the extent of about 0″·4 or 0″·5 since 1848 seems to be well established.

"*Wateringbury, May* 1, 1855.

Observation of an Occultation of Venus by the Moon.
By J. Ferguson, Esq.

(*Letter to Lieut. Maury, U.S.N., Superintendent of the National Observatory, Washington, communicated by Lord Wrottesley.*)

" I submit the following results and description of the occultation of the planet *Venus* by the moon, observed with the large equatoreal on the 18th of the present month. The following are the meteorological indications for the time of observation taken from the journal of the Observatory,—

Bar. 29·912.   Therm. attached, 75°.   Ext. therm. 70°·5.

" The night was sultry, with a damp atmosphere, but quite serene and clear, till within 15 degrees of the horizon, below which was a brownish haze, as of Indian summer. The dark limb of the moon was distinctly seen with the naked eye until after the occultation. After 8 o'clock the planet became ill-defined, having a reddish-purple tinge. The power of the eyepiece used was 120, with which the following times were observed:—

" At 8^h 37^m 30^s M.T. Wash.—The limbs were in contact. This time is uncertain two seconds, owing to the flame-like edge of the planet, the moon's limb being well defined.

" At 8^h 39^m 10^s.—The planet was seen half its diameter within the limb of the moon, exhibiting no diminution of light or of magnitude, but showing as if it were on this, and not on the other side of the moon.

" At 8^h 39^m 38^s.—The first diminution of magnitude was apparent, the inside or cut edge being straight and well defined, the planet still showing as if projected on the surface of the moon. The last two phases were observed with great care.

" At 8^h 40^m 7^s.—The immersion, which was sudden, but not instantaneous.

" *U.S.N. Observatory, April* 19, 1855."

Account of the Operations for determining the Longitude of Fredericton, New Brunswick, by Galvanic Signals. Extracted from a Report to the Lieutenant-Governor.

(*Communicated by the Astronomer Royal.*)

" The Government of the United States has spared neither pains nor money in determining by the most approved astronomical methods, and by interchanges of upwards of one thousand chronometers, the difference of longitude between Greenwich and Harvard College Observatory, in order that the latter might

serve as a point of reference in conducting the operations of the
Coast Survey. By our telegraphic communication with Boston,
and through the kind co-operation of Professor Bond and his
assistants, we have, at a comparatively insignificant amount of
trouble and expenditure, been enabled to avail ourselves of the
labours undertaken for the above-mentioned purpose, and thus to
ascertain the longitude of Fredericton with probably an equal
degree of precision.

"It was originally intended to have an unbroken telegraphic
communication between the Fredericton Observatory and that of
Harvard University, but in consequence of the wires from the
latter to the office in Boston being out of repair, Professor Bond
found it necessary to trust to two excellent sidereal micrometers
for the interval; and remarks, that on examination he was induced
to believe that no greater error had arisen from this source than
would have taken place had the communication been made from
the room adjoining the transit instrument. Professor Bond's
chronometers were carefully and repeatedly compared with his
transit-clock and with each other, both before and after inter-
changing signals, so as to ascertain their error and rate; and at
both observatories, on each day of operations, the meridian passages
of a number of stars were observed, in order to obtain the error
and rate of the transit-clocks.

"On the evening of the 23d of January, 1855, we received
the first series of signals from Boston. Mr. Coolidge (Mr. Bond's
assistant) commenced at an even minute by his chronometer, and
sent us second-beats for fifty consecutive seconds. This was
continued for ten successive minutes, beginning always at the
even minute, and we carefully noted the times by our transit-
clock. On examining all, we found that the times of the first
signal would be as exhibited in

Table I.

	Clock Time.	Clock Error +.	True Sidereal Time.
	h m s	m s	h m s
At Fredericton	6 29 37·7	1 40·18	6 27 57·52
Cambridge	6 10 21·5	0 21·36	6 10 0·14
		Hence the difference of longitude	0 17 57·38

"On the evening of the 2d of February we took the initiative,
and sent a series of signals to Boston, the result from which is
given in

Table II.

	Clock Time.	Clock Error +.	True Sidereal Time.
	h m s	m s	h m s
At Fredericton	6 38 0	2 25·88	6 35 34·12
Cambridge	6 18 13·3	0 36·48	6 17 36·82
		Hence the difference of longitude	0 17 57·30

" On the same evening we sent another series, and the result deduced from them is shown in

Table III.

	Clock Time.	Clock Error +.	True Sidereal Time.
	h m s	m s	h m s
At Fredericton	6 49 0·0	2 25·88	6 46 34·12
Cambridge	6 29 13·3	0 36·48	6 28 36·82

Hence the difference of longitude   0 17 57·30

" On the same evening we received from Cambridge a series of signals, which give a result exhibited in

Table IV.

	Clock Time.	Clock Error +.	True Sidereal Time.
	h m s	m s	h m s
At Fredericton	7 4 23·6	2 25·88	7 1 57·72
Cambridge	6 44 37·0	0 36·48	6 44 0·52

Hence the difference of longitude   0 17 57·20

" On the evening of the 10th of February we were again in telegraphic communication with Boston, and the result of the first series of signals which were sent from Fredericton and recorded at Boston is exhibited in

Table V.

	Clock Time.	Clock Error +.	True Sidereal Time.
	h m s	m s	h m s
At Fredericton	7 1 0	3 23·7	6 57 36·3
Cambridge	6 40 27·05	0 47·89	6 39 39·16

Hence the difference of longitude   0 17 57·14

" The second series of signals on the same evening was transmitted from Boston and recorded at Fredericton, and the result is as shown in

Table VI.

	Clock Time.	Clock Error +.	True Sidereal Time.
	h m s	m s	h m s
At Fredericton	7 17 21	3 23·7	7 13 57·3
Cambridge	6 56 48	0 47·89	6 56 0·11

Hence the difference of longitude   0 17 57·19

" We next sent a series of signals to Boston, the result derivable from which is given in

Table VII.

	Clock Time.	Clock Error +.	True Sidereal Time.
	h m s	m s	h m s
At Fredericton	7 43 0	3 23·7	7 39 36·3
Cambridge	7 22 27	0 47·89	7 21 39·11

Hence the difference of longitude   0 17 57·19

" We then received from Boston and recorded at Fredericton another series of signals (the fourth of the same evening), the result of which is shown in

Table VIII.

	Clock Time.	Clock Error +.	True Sidereal Time.
	h m s	m s	h m s
At Fredericton	7 51 21	3 23·7	7 47 57·3
Cambridge	7 30 48 (17·49 × 30·4 ◾) 0 47·89		7 30 0·11

Hence the difference of longitude 0 17 57·19

" And, lastly, we received from Cambridge a single tap for the purpose of comparing clocks, and the result deducible from it is exhibited in

Table IX.

	Clock Time.	Clock Error +.	True Sidereal Time.
	h m s	m s	h m s
At Fredericton	8 7 21	3 23·7	8 3 57·3
Cambridge	7 46 48	0 47·89	7 46 0·11

Hence the difference of longitude 0 17 57·19

" On examining the operations of February 10, it will be perceived that the second-beats of the Boston chronometer and the Fredericton transit - clock continued synchronous through-out, and, therefore, must have had the same rate. Hence the same clock-errors are applicable to the whole of the series for the evening. We may remark, that the results obtained from this last night's work are considered the most complete and satis-factory, and from them alone the difference of longitude would be 0^h 17^m $57^s·18$. If, however, we take the mean of all the operations, the difference would be 0^h 17^m $57^s·23$; and as Cam-bridge Observatory is 4^h 44^m $30^s·66$ west of Greenwich, it follows that the longitude of Fredericton is 4^h 26^m $33^s·43$ west of Green-wich. Converting the above time into arc, we have,—

Longitude of Fredericton .... 66 38 21·5
The Crown Land Department makes it 66 37 54

Difference 0 0 27·5

" This difference is smaller than could have been anticipated, or than we should have been warranted in assuming.

" J. B. TOLDERVY.
" W. B. JACK.

" *Fredericton, March* 5, 1855."

The following is an extract of a letter from Professor Jack to the Astronomer Royal, dated Fredericton, April 19, 1855, in re-ference to the foregoing determination :—

" Morse's machine was employed, and as the armature of the magnet at one end of the line has, on the connexion being made

by means of the finger-key at the other, to move through a space not probably exceeding the $\frac{1}{300}$ part of an inch, and as the con-tact of the armature and magnet gives a distinct sound, which can be easily compared with the second-beats of the transit-clock, we cannot but think this method susceptible of greater accuracy than that which depends on watching the movements of a needle. Still greater accuracy could have been obtained had we been supplied with Bond's self-registering apparatus."

The following is an extract of a letter, dated April 23, 1855, received from George Hamilton, Esq. F.R.A.S., Egremont, near Liverpool, in reference to Mr. Elliott's invention of a Mechanical Imitation of Precession, alluded to in the last number of the *Monthly Notices:*—

"In the year 1842, Mr. Edward Davis, optician, then of Liverpool, now, I believe, of Shrewsbury, constructed for me an apparatus precisely the same in principle as Atkinson's, and pre-cisely the same in form as Elliot's. I have used the apparatus since that time to illustrate precession of the equinox and nutation of the earth's axis. My apparatus was made after a model shown to me by Mr. Elliot, and, judging from the appearance of his apparatus, I should conclude that it had been constructed many years before."

Elements of Leucothea.   By M. Bruhns.

Epoch, 1855.   May 0·0, M.T. Berlin.

$$M = 351° \; 16' \; 22''·7$$
$$\left. \begin{array}{l} \varpi = 212 \; 55 \; 38·7 \\ \Omega = 355 \; 42 \; 15·2 \end{array} \right\} \; M \; \text{æquinox, } 1855·0$$
$$i = 7 \; 37 \; 35·9$$
$$\varphi = 14 \; 17 \; 36·9$$
$$\mu = 694''·479$$
$$\log a = 0·472232$$

These elements were calculated from an observation made at Bilk, dated April 20; and two Berlin observations of April 27 and May 3.

Note relative to a Phenomenon seen in the Planet Venus.
By the Rev. W. R. Dawes.

"In the *Monthly Notices* for March 1854, there is a notice of some observations of the planet *Venus*, when near her inferior conjunction, by Mr. Guthrie. The phenomenon he observed is

very interesting; and it seems desirable that some more definite statement should be furnished by Mr. Guthrie respecting the observations, giving the exact *date*, the approximate *altitude* of the planet, or the *time* of observation from which the altitude and the degree of twilight might be computed.

"It is to be regretted that a notice of the observations was not published at the time, as it might have led to a repetition of them by other observers on subsequent opportunities.

"I confess that I have frequently examined *Venus* near her inferior conjunction, with the view of ascertaining if any such phenomenon were visible, as her apparently dense atmosphere might seem to render probable, but have never caught sight of anything which could be supposed to arise from that cause, except a very moderate elongation of the extremities of the crescent.

"*Wateringbury, April* 30, 1855."

[At the time when Mr. Guthrie's Note was communicated to the Society the Editor endeavoured to obtain some precise details respecting the phenomenon to which it refers, but it was found that Mr. Guthrie did not possess any written record of his observations. He merely remembered that they were made on the occasion of the inferior conjunction of the planet in December 1842. This was not, indeed, explicitly stated by him, but was inferred from the terms of his letter.]

Extract from a Paper by Eyre B. Powell, Esq. on the Orbit of α Centauri. (See *Monthly Notices* for January last, p. 87.)

"I have seen no other orbit of α *Centauri* than the one calculated by Captain Jacob, and appended to the Poonah Catalogue of Double Stars: between his and the present there is a great resemblance as to all the elements, save γ, Ω, and a, but in these we differ *in toto*. A table of variations, which I computed, explains our discrepancies: in the angles of position, like variations of γ and Ω, produce opposite changes, such that, from 1826 to 1848, a decrease of the inclination can be to a very great extent compensated by a retrogradation of the line of nodes; in the distances also there is a similar adjustability among different elements, the variations of a, γ, and Ω, playing the most prominent parts. The evident proximity of the primary star to the following side of the projected ellipse, and the necessary area of the perspective orbit, seem to mark that the line of nodes must lie nearly in the meridian, and if so, γ must possess a large value. With my second set of elements the area of the projected ellipse

* These remarks would have been inserted in the *Monthly Notices* for April, along with those of Captain Jacob on the same subject, had not the Paper which contained them been at the time in the hands of the printer for publication in the volume of Memoirs.—ED.

comes out 154 square seconds, and the sector between Captain Jacob's observations of 1846 and 1854, taken in connexion with a period of 75·3 years, gives an area of 153·6 square seconds for the ellipse.   The elements also represent the observation of Fallows, as to position, with an error of only + 44′, and that of La Caille, as to distance, with the trifling error of —″·34."

Notes on the Management of Chronometers and the Measurement of Meridian Distances.   By Captain Charles F. A. Shadwell, R.N. C.B. &c.   London, 8vo. 1855.

The author states that this work is mainly designed for the use of officers of the Royal Navy, but he expresses a hope that it may be perused with advantage by intelligent individuals of the Mercantile Marine.   His object has been not so much to show how Chronometers may be rendered subservient to the ordinary purposes of Navigation as to exhibit their application to the advancement of Maritime Geography.

The work is divided into nine chapters, the first three of which are devoted to various preliminary details relative to the management of chronometers and to the determination of local time.   In the fourth chapter the author explains the mode of ascertaining the rates of chronometers by two observations of a similar kind taken at a convenient interval.   On this part of his subject he remarks :—" For the determination of the error on local mean time, it has formerly generally been the practice in the measurement of meridian distances, or on taking a departure from a port, to adopt as the starting-point the error shown by the last observation used in the determination of the rate ; but inasmuch as in the deduction of the latter, we necessarily place equal confidence in both the observations, there seems to be no good reason why we should not do the same in assuming the error and adapting as our *working* error the mean of the two errors on which we have already agreed to make the rate depend.

" The *mean* error thus introduced may probably be assumed to be more accurate than either of the single elements on which it depends ; while this mode of proceeding, moreover, will have the advantage of referring both the error and the rate to the same epoch."

The author remarks that this mode of viewing the subject has the advantage of simplifying the process for finding the corrections to be applied for the changes of rate which may have taken place in the transit from port to port.

The principles explained in this chapter are illustrated by a copious collection of examples.   In one of these the error and rate of the chronometer are determined from observations of a *dissimilar* kind made in two places whose *difference of meridian is known*.   " This example," says the author, " affords an illustration of an important remark of Raper's, ' that as the longitudes of the

several places approach to precision, ships will employ the differ-
ence of longitude as a means of obtaining directly the *sea-rates* of
their chronometers, instead of waiting to obtain harbour-rates;
thus exemplifying one of the most important ends to which the
perfection of hydrography can serve.' "

Upon the question with respect to the best interval for deter-
mining the rates of chronometers, the author justly remarks that
if the stability of the rate could be relied upon, its value would be
obtained with greater accuracy the longer the interval of time
included between the observations for ascertaining the values of
the error on mean time.

" In practice, however, this theoretic view is limited in its ap-
plication by the impossibility of depending confidently on the
steadiness of the rate over long periods, and by the consequent
necessity for checking the performances of chronometers by fre-
quent determinations of their errors, and thus breaking up the
intervals on which the rates depend into short periods.

" As a matter of practice, therefore, it seems advisable when
circumstances permit, that the rate of a chronometer should not
depend on observations made at an interval of *less* than *five* or
more than *ten* days.   Seven days will be found a convenient ave-
rage interval; and in the case of eight-day chronometers, more-
over, it embraces the period affected by the whole weight of the
chain.

" With the above limitations it may be laid down as a maxim
that chronometers cannot be rated too often when time and oppor-
tunity permit.

" It seems advisable, moreover, when the measurement of me-
ridian distances is in contemplation, that in so far as may be prac-
ticable, the two rates employed should depend on observations
made at *equal* intervals of time; since, when the intervals are
very unequal, the small errors of observation do not exercise an
equal influence on the final results, and their values are unduly
affected by the errors of observation attendant on the rate deter-
mined at the shorter of the two periods."

In the fifth chapter the author explains the mode of obtaining
the errors and rates of chronometers by a combination of several
observations taken within a convenient interval of time.   The
accumulations of the rate for the partial intervals form the
groundwork of a series of equations of condition, which are solved
by the method of least squares.   On the question with respect to
the advantage attending the application of this method, the author
remarks, that " although in most cases the mean arithmetic error
corresponding to the mean of the times of observation may be
considered as sufficiently accurate and convenient for practice;
yet in cases where an examination of the observations seems to
indicate considerable instability or fluctuation of rate during the
period of rating, and when the computer does not object to the
additional labour involved in the latter more elaborate process,
there is no doubt its results will usually be more satisfactory, and

certainly more correct, while it will fully repay the extra trouble employed in its manipulation."

In the sixth chapter the author treats of the chronometric determination of meridian distances, explaining the different hypotheses which have been adopted for finding the variation of the rate during the voyage between the two places where the rate has been ascertained. The following chapter contains a copious collection of examples illustrative of the principles laid down in this chapter.

In the eighth chapter, the author shows how the difference of longitude between two places may be obtained from observations giving the *errors* of the chronometers at the two stations, independently of a knowledge of their preceding or subsequent *rates*. The *error* of the chronometer is ascertained: first, upon starting from the first station; secondly, upon arriving at the second station; thirdly, upon quitting the second station; and, lastly, upon arriving again at the first station. From these data the difference of longitude between the two stations, and the rate of the chronometer for the interval included between the departure from and subsequent arrival at the first station, termed the *travelling rate*, is deduced by a formula for which the author expresses his obligation to the Rev. George Fisher.

The ninth chapter of the work is devoted to a series of precepts on the mode of recording the results of chronometric measurements.

It may be remarked, in conclusion, that the subject-matter of the work is arranged in a very lucid order, that the explanations are concise and simple, and that they are in every instance illustrated by numerous examples. Possessing such claims to consideration, the work cannot fail to prove a most valuable manual to the naval profession in general, and more especially to the class of persons whose instruction the author had mainly in view while engaged in preparing it for publication.

Captain Shea has forwarded for inspection a volume containing the record of his observations of the solar spots. The appearances and disappearances to which he refers seem to be no other than the ordinary phenomena depending on the rotation of the sun upon its axis.

An azimuth compass for determining the variation was exhibited by Baron Kleinsorgen. It is assumed that the time of noon is known or may be determined on board, and consequently the direction of the true meridian at that moment. A line is stretched across the compass-box, and this box is turned until the shadow of the line coincides with the diameter of a fixed plane a little distance below it. The pin on which the card turns is

connected with this plane, and there is an index corresponding with the diameter which points out the reading of the compass for the time of observation. Possibly in those merchant-ships where the altitude of the sun at noon is the only observation made, such an instrument might give useful warning to the helmsman, but the method only admits of a loose approximation.

New Variable Star.

Professor Argelander, in a letter to the Editor of the *Astronomische Nachrichten* (*Astron. Nach.*, No. 9580), announces the discovery of a new variable star, namely, the star W. VI. No. 1911 = Lal. No. 13825. It is represented by Lalande as an 8^m; by Bessel, once as an 8^m, and once as an 8.9^m. Fellöcker marks it in the Catalogue as an 8^m, but upon the Chart as an 8.9^m. Bremiker makes it a 9^m. However, on the 4th and 5th of March, 1854, and on the 4th of March, 1855, it was not visible in the comet-searcher, nor was it seen in the heliometer on the 30th of the same month; but it must be admitted that on this last occasion the sky was unfavourable for observation. Two days afterwards, however, it appeared like a faint star of the 9.10^m; and on the 11th and 12th of April it shone like a faint 9^m. It is of a very ruddy colour, and, like many other variable stars of small magnitude, appears to have a period of rather more than a year. Professor Argelander proposes to call it *R Canis Minoris*, and expresses his intention of following it as long as possible.

On several Stars which have Disappeared from his Ecliptical Charts. By M. Chacornac.

" On the 7th of August, 1852, at 15^h, I placed upon one of my charts, in $21^h 36^m\cdot5$ of right ascension and $+ 14° 33'\cdot9$ of declination, a star of the seventh magnitude, which was situate between two stars of the ninth magnitude. The position of this star was obtained by comparisons made with another star of the eighth magnitude in its vicinity.

" Next day I only compared with the heavens the regions of the chart where small stars were found to be situate; unfortunately, the region in which the star of the seventh magnitude had been placed was almost entirely wanting in small stars. I did not, therefore, verify it on that day, expecting on a future occasion to fill it up with small stars, and to re-observe at the same time the star which I had placed there.

" On the 20th of the same month, having finished this chart, I was very much surprised not to find again the star of the seventh magnitude, while those of the ninth magnitude were found to be perfectly in the place which my chart assigned to

them. Convinced by this circumstance, and by the verification of the numerical results obtained on the 7th of August, that there could be no mistake, I undertook forthwith the search of this star upon the supposition that it was a planet, describing its arc of retrogradation. With this object in view, I constructed a rough draught of the neighbouring stars as far as the ninth magnitude. On the 30th of August this chart already comprised in the direction of the retrogradation 14° of right ascension, and extended on each side of the ecliptic 8° to 10° of latitude, without obtaining any result. I then learned that Mr. Hind had just discovered the planet *Melpomene* in the very region which I was exploring. This planet being fainter than the stars with which I was occupied, I immediately abandoned the search of the star of the seventh magnitude, which I supposed to be variable, in order to resume the construction of my charts. Since that time the star has not reappeared.

"On the 30th of December, 1852, I placed upon my charts, in $8^h 47^m\cdot 3$ of right ascension and $+ 17° 44'$ of declination, a star of the ninth magnitude. I verified the position of this star only twelve months afterwards; that is to say, on the 4th of December, 1853. It was then invisible, and has not since reappeared. This star was near another very red star of the sixth magnitude.

"On the 5th of July, 1853, I also placed on a rough draught, in $16^h 8^m\cdot 8$ of right ascension and $- 22° 21'$ of declination, a star of the ninth magnitude near a nebula; these stars were not verified in the same year.

"On the 19th of May, 1854, upon completing this chart, I noticed that this star of the ninth magnitude had disappeared from the place where it was observed. It remained invisible till the month of August of the same year. Being sought for again on the 4th of April, 1855, it was found to be of the tenth magnitude.

"On the 8th of April, 1853, I observed a star of the eleventh magnitude in $11^h 3^m\cdot 3$ of right ascension and $+6° 54'$ of declination. This star had precisely the same right ascension as another star of the ninth magnitude which was very near to it. On the 15th of the same month it was invisible. Having been again searched for on the 16th and 17th as a planet, I was not able to find it, either on account of the brightness of the moon or in consequence of its inconsiderable motion. On the 18th I knew that it was the planet *Themis* which M. De Gasparis had just discovered.

"On the 27th of December, 1853, I observed between two stars, very close to each other, between the eighth and ninth magnitudes, a star of the tenth, which I had not hitherto noticed. This chart not being filled up with small stars, I did not attach any other importance to the observation than that suggested by a multitude of new stars which are encountered in observations of this kind. I, therefore, placed this star of the tenth magnitude upon my chart, writing beside it at full length *triple star*.

"This triple star was only verified on the 26th of March, 1854.

It was then only double, the small star of the tenth magnitude having disappeared. I examined this double star with a high magnifying power, and I again searched for the star which had disappeared upon the supposition of its being a planet, but without result. The following is the position of the double star:—$4^h 14^m\cdot6$ of right ascension $+23° 58'$ of declination.

" On the 30th of December, 1853, I observed a star of the eleventh magnitude in $3^h 33^m\cdot7$ of right ascension and $+20° 51'$ of declination. The unfavourable state of the weather prevented me from verifying its position with certainty till the 20th of January, 1854; it was then invisible. I have searched for it again as a planet, but without success. It has not reappeared.

" On the 10th of January, 1854, I found that a star of the eleventh magnitude, which must have been placed upon my charts between the 4th of September and the 29th of November, 1853, in $4^h 26^m\cdot9$ of right ascension and $+21° 26'\cdot8$ of declination, had disappeared. I searched for it again in my charts as a planet without result. It has not reappeared.

" On the 26th of January, 1854, I undertook the construction of a rough draught, upon which I placed very few stars during the evenings of the 30th and 31st of January. I did not resume the construction of this chart till the 30th of July of the same year: two stars had then disappeared from it. The one of the eleventh magnitude was not reobserved till later with the great refractor of the Observatory of Paris; it was then only of the thirteenth magnitude. The other of the eleventh magnitude has not been reobserved. The latter was situated in $23^h 27^m\cdot5$ of right ascension and $-4° 15'$ of declination.

" On the 19th of July a star of the tenth magnitude was placed upon my charts in $21^h 7^m$ of right ascension and $-15° 5'$ of declination. It was the planet *Urania* found by Mr. Hind. On the same day I found that a star of the ninth magnitude which must have been placed upon the chart in the course of the month of January 1854, in $21^h 20^m\cdot2$ of right ascension and $-12° 53'$ of declination, had disappeared.

" This star, which had been observed during the twilight, formed with other stars of the ninth magnitude a remarkable configuration. It was a little more brilliant than another star also of the ninth magnitude, which was very near to it.

" On the 11th of January, 1855, I found that a star of the eleventh magnitude, which had been placed on the 26th of October, 1854, upon one of my charts in $7^h 30^m\cdot3$ of right ascension and $+23° 54'\cdot7$ of declination, had reappeared. I was not able to reobserve this star earlier on account of the unfavourable weather which prevailed at Paris during the whole season of autumn. It has not reappeared.

" On the 28th of October, 1854, I found that a star of the eleventh magnitude, placed upon my charts in $2^h 47^m$ of right ascension and $+17° 32'\cdot5$ of declination, had disappeared on the 26th of September in the same year.

" It was a position of the planet *Pomona,* which **M. Goldschmidt** had just discovered.

" In the ninth hour, and quite recently, two stars have also disappeared from the positions which my charts assigned to them.

" One of the tenth magnitude which was placed upon these charts towards the end of the year 1854, has been invisible since the 17th of January, 1855. The other, which was of the eleventh magnitude, was observed on the 25th of January of the same year, and placed upon my charts in $9^h\ 27^m \cdot 2$ of right ascension and $+ 16° 8' \cdot 7$ of declination. It had disappeared on the 19th of March, at which date only I was enabled to verify its position.

" These two stars have been searched for again as planets and variable stars, but without result.

" On the 5th of October, 1853, I perceived a star of the twelfth magnitude in a region wherein my chart did not exhibit any. This chart was, however, pretty well filled up. I affixed a mark to this star and proceeded on the following day to verify its position. Clouds permitted only a short verification, during which I found that the star had disappeared from the place where I had observed it on the preceding evening, but I was unable to find it again in the neighbouring region. I searched for it again on the following days with great care, but without success. Since then it has not been found in the position of the 5th of October, which was this,—

$$\text{R.A.} = o^h\ 44^m \cdot 4 \qquad \text{Decl.} = + 8°\ 46' \cdot 2$$

" In order that it may be understood how I have allowed so many stars to escape from me, I ought to mention that, in order to construct my ecliptic zones with rapidity,—although I always had the intention of verifying on the following evening the position of the stars observed on any preceding evening,—I have frequently been compelled by the unfavourable change in the weather to defer the verifications to other epochs, and to profit by the beautiful nights in occupying myself especially with the regions of the ecliptic in opposition to the sun. I ought also to mention that it has happened to me, at Paris especially, to have been obliged to postpone for several months similar verifications by the coincidence of the rare instances of a clear sky with the full moon; for it is known that when the question relates to the small planets, the great brightness which the moon diffuses over the entire heavens, when it is approaching opposition, effaces their light as effectually as the close proximity of that body. The usually not very serene condition of the sky at Paris, the inconsiderable altitude of these stars above the horizon, are so many causes which prevent the searching for and verifying of them as often as is possible in the case of stars of a higher magnitude.

" *Imperial Observatory, Paris, April* 15, 1855."

Astronomical and Meteorological Observations made at the Rad-cliffe Observatory in the Year 1853. Under the Superin-tendence of Manuel J. Johnson, M.A., Radcliffe Observer. Vol. XIV. 8vo. Oxford, 1855.

The principal part of this volume is devoted to the class of observations required for the construction of the Circumpolar Catalogue. An appendix to the volume contains the observations with the heliometer. In the introduction to this part of the work Mr. Johnson gives the details of his investigation of the paral-laxes of 61 *Cygni* and 1830 *Groombridge,* to which allusion was made in the Annual Report of the Council for this year. With respect to the observations with the heliometer, a few consist of observations of the diameters of the planets; but they chiefly refer to double stars, and to measurements of the relative positions of a few select stars for parallax. The latter class of stars includes *Arcturus* and *α Lyræ.*

New Comet.

A comet was discovered by M. Klinkerfues at Göttingen on the 4th instant. The following position of it was obtained by M. Rümker at Hamburgh on the next evening :—

	Hamburgh M.T.	Comet's R.A.	Comet's Decl.
	h m s	° ′ ″	° ′ ″
1855, June 5	10 49 37·0	107 50 58·7	+36 15 53·5

Instruments for Sale, formerly belonging to the late Bryan Donkin, Esq.

A 3½ in. achromatic telescope and claw-stand, by Tulley	£35	0 0
A position micrometer, by Troughton	4	0 0
A mountain barometer, by Troughton	7	0 0
Ditto, by Bautern	3	0 0
A 30-in. transit and stand collimator, and a Donkin level for ditto	15	0 0
A sextant and stand, with bottle, &c., by Troughton ..	14	0 0
A 12-in. theodolite, plates and legs, by Troughton ..	10	0 0
A Y level, 30 inches, by Simms	6	0 0
A level, 20 inches and legs.	5	0 0
A dynameter, by Dollond	2	0 0
Two 2-feet pentagraphs, one by Troughton	3	0 0
A small solar microscope	1	0 0
A perspective machine	1	0 0
An equatoreal block	1	0 0
A grinding tool, by Holtzapffel	1	0 0
A hygrometer, by Daniel	1	0 0

They may be seen at the rooms of the Royal Astronomical Society.

Instrument for Sale.

Object-glass, by Lerebours and Secretan of Paris ; focal length, 86·3 in. ; aperture, 6·2 in. ; not quite perfect, having striæ at two points of the circumference. The central four inches are very good. The measures of 144 double stars given in the last volume of *Madras Observations* were taken with this glass. Price 28*l*. Inquire at the apartments of the Society.

A Lassell's machine for grinding and polishing specula for reflecting telescopes, made by Mr. Nasmyth of Patricroft. Price 30*l*. Inquire at the Society's apartments.

ERRATA.

The upper line of the figure X, referred to by Captain Jacob in his Paper on the Orbit of *a Centauri*, inserted in the *Monthly Notices* for April (p. 179), instead of being *rectilinear*, as incorrectly drawn by the engraver, should have been *slightly curvilinear*, so as to adapt itself to the periphery of the ellipse.

Page 181, line 4 from bottom, *for* 52° 24′ 52″, *read* 52° 54′ 52″.

CONTENTS.

M. J. Johnson, Esq. President, in the Chair.

Lieut. Tennant, Bengal Engineers, India;
Rev. S. Newth, New College, London; and
Capt. W. Noble, R.A., Woolwich,

were balloted for and duly elected Fellows of the Society.

On the Computation of Double Star Orbits. By Capt. W. S. Jacob,
East India Company's Astronomer.

" In bringing this subject to the notice of the Society, it is
proper to premise that I have no decidedly new method to
propose; but it has occurred to me, having computed many orbits
in various ways, that my experience may be of some service to
others, who may feel disposed to take up the subject, by sug-
gesting certain facilities in minor matters of detail, by which time
may be saved or greater accuracy attained.

" The plan I follow is, in the main, Herschel's first method
given in vol. v. of the Society's *Memoirs;* his second, or improved
method, in vol. xviii. I have also tried ; but, though more elegant
and scientific in form, it does not appear to me to insure greater
accuracy, while it has the disadvantage of working *in the dark,*
instead of allowing scope for the judgment to be exercised at every
step, which, considering the loose character of much of the mate-
rials to be dealt with, appears to me of great importance. In
order that this second method may be advantageously applied, the
observations would have to be all of about the same order of
goodness, and pretty equally distributed round a considerable part
of the periphery of the ellipse, — conditions which are scarcely as
yet attainable in a single instance; and it would also be essential
that all the computations should be made in duplicate and com-
pared at every step, since a small numerical error would vitiate
the whole results, and would be by no means easy of detection.

" I have, therefore, after a few trials, abandoned this method, and returned to the old or graphical mode with very slight alterations. In the preliminary operations up to the first approximation to the apparent orbit, Herschel's rules are exactly followed; occasionally, when the distance-measures have been sufficiently good and numerous, I have projected them, as well as the angles, into a curve, and have brought the two curves into unison by slight alterations and successive trials; but the occasion for this has rarely offered, and it is in general easy to introduce the effect of the distances in the final corrections. Having then laid down the corrected places from the position curve, I cut out in paper an ellipse of about the proper size, and adapt this so as to agree as nearly as possible with the points laid down; it is then easy to see, not only the proper direction of the major axis, but also whether any variation is required in the dimensions of the ellipse: having thus got approximately the apparent orbit, the real elements are obtained by Herschel's rules, with the following exceptions: having found a' and α graphically, I find it lead to greater accuracy to compute b' and β by the following formulæ rather than to get *them* also by the graphical mode:—

$$\tan B = \frac{t^2}{c^2} \tan A$$

$$a' = \frac{t \cdot \sec A}{\sqrt{1 + \dfrac{t^2}{c^2} \tan^2 A}}$$

$$\frac{a'}{b'} = \frac{t \cdot \sec A}{c \cdot \sec B}$$

where t and c are the transverse and conjugate axes of the apparent ellipse, a' and b' the projected major and minor axes of the true orbit, A the inclination of a' to t, and B that of b' to c.

" To obtain the mean motion and epoch, Herschel proposes two methods: first, to cut out of card and weigh in a nice balance the whole ellipse and the portions included between the extreme dates of observation and the projected major axis; or second, analytical, by the resolution of certain equations: the first appears to have been the plan generally preferred; the second having been probably rejected by common consent as needlessly laborious. I have not followed either, not having, in general, had at hand a balance accurate enough for the purpose, and being also doubtful of the advantage of the statical mode over that of simple mensuration:—whether, even supposing the weights could be obtained with perfect accuracy, it would be possible to cut out the ellipse more exactly, or even so exactly as it could be measured. My plan is to divide the portion of the ellipse included between the observations into triangles and segments, and measure them in the ordinary way: the area of the elliptic segment being approximately $= 0.67\, ch$, where c is the chord and h the height or versed sine of the segment; this is exact for the parabola, and

practically so for the ellipse when the segment is small enough not to differ sensibly from that of a parabola,—say, in general, when *c* exceeds 10 *h*. Supposing the observations spread over a large portion of the ellipse, so as to include within them (or nearly so) the area of one or more quadrants, it will evidently be sufficient to measure the *difference* of the total area described from that of the included quadrants.

"For drawing ellipses I have found the best mode, both as regards convenience and accuracy, to be that of co-ordinates, depending on the fact that all ellipses with a common axis (major or minor) have the ordinates on the same abscisses proportional to the *other* axis; consequently, since the circle is one of them, if the absciss* $x = a \sin$ X, then $y = b \cos$ X.

"There are several of these co-ordinates which can be easily retained in the memory, so that no time need be lost in referring to tables; thus,—

$$\text{If } x = a \times \cdot 28 \quad \text{then } y = b \times \cdot 96$$

·50	...	...	...	·866
·60	...	...	...	·80
·707	...	...	...	·707
·80	...	...	...	·60
·866	...	...	...	·50
·96	...	...	...	·28

"It will be observed that there are only 4 ratios to be remembered, and they furnish 7 points in each quadrant, besides the extremities of the axes, or 32 points in the whole periphery, through which the curve may be readily drawn with a curve-ruler, such as is used by architects for tracing their mouldings.

"For approximately solving the equation $u - e \cdot \sin u = m$, Sir J. Herschel mentions having constructed a machine of wheel-work: this is, doubtless, a very ingenious contrivance, and probably useful for other purposes; but it is not at all essential here, since a solution may be obtained by the common slide-rule. The line of sines on a Bate's 10-in. rule will give the value by simple inspection to about o°·1, or even less; whereas the machine was said to give it only to the nearest degree.

"There is nothing further to be noticed till we come to the final corrections. After comparing the computed with the observed places, I formerly used to correct the elements by the method of least squares. This proved unsatisfactory; the labour was great, and the result by no means commensurate, since the changes required in the elements were often so large as to render sensible the effect of variations of the second and higher orders, and it was not easy to introduce these into the equations of condition; and I now adopt a partly graphical mode, by laying down carefully on a large scale the apparent orbit from the computed places. This will not agree exactly with the one first assumed,

* x having its origin at the centre.

because slight and obvious corrections will probably have |
introduced into the elements (especially Ω) in the course of c
putation: the eye will then readily detect the alterations requ
in the course or dimensions of the curve to make it agree 1
closely with observation, and it will sometimes be an assistanc
cut out in paper an ellipse of about the right size, and see whe
by turning it this way or that the proper amount of deviation
be given.   In this way, after a few trials, a result will be obta
as good as the materials are capable of giving.

"It will generally be advisable to compute the coefficien-
variation for the different elements, to serve as guides in ma
these final alterations.   They are very easily computed as follow

$$\frac{\Delta \theta}{\Delta \tau} = \text{annual motion (apparent)}$$

$$\frac{\Delta \theta}{\Delta n} = \text{annual motion} \times \frac{t - \tau}{n}$$

$$\frac{\Delta \theta}{\Delta e} = \frac{\text{annual motion}}{n} \times \sin u \left(1 + \frac{\sin u}{2 \sin v . \sqrt{1 - e^2}} \right)$$

$$\frac{\Delta \theta}{\Delta (\log \cos \gamma)} = \frac{1}{\text{tab. diff. log tan } (\theta - \Omega)}$$

$$\frac{\Delta \theta}{\Delta \lambda} = \frac{\text{tab. diff. log tan } (v + \lambda)}{\text{tab. diff. log tan } (\theta - \Omega)}$$

"The annual motion is easily deduced from the compu
change of angle for any given interval, and the mean distance
that interval, remembering that the rate of motion is always
the inverse ratio of the squares of the distances.

"The coefficients thus obtained may be employed, if it
thought fit, in forming equations of condition for solution by
method of least squares, but it will in general be found prefer
to use them, in conjunction with the graphical mode above
scribed, to correct the elements by trial and error; for hav
found graphically the general direction of the alterations requi
the coefficients will enable us to judge on which of the elem
the changes can most advantageously be thrown, so as to prod
the greatest proportional effect on the resulting angles.

"The following elements for the orbit of γ *Coronæ Austr*
were obtained in the manner above described, being the resul
the second trial:—

$$\tau = 1863^{r}{\cdot}08$$
$$P = 100{\cdot}80$$
$$n = 3°{\cdot}5715$$
$$\varpi = 256° \ 12'$$
$$\Omega = 352 \ 13$$
$$\lambda = 266 \ 25$$
$$\gamma = \ \ 53 \ 35 \quad \text{l. cos} = 9{\cdot}7736$$
$$e = \ \ 0{\cdot}602$$
$$a = \ \ 2''{\cdot}549$$

Comparison.

Date.	ϑo.	ϑc. — ϑo. ∠	ϑc. — ϑo. arc.	ρo.	ρc — ρo.
	o '			'	
1834·47	37 6	+46	·034		
1835·55	36 48	—48	·035		
1836·43	34 30	— 3	·002		
1837·43	32 42	+ 1	·001	2·66	—·14
1847·32	14 6.	+ 5	·003	2·30	—·01
1850·46	5 52	+77	·047	2·29	—·20
1851·54	4 28	— 3	·002	2·26	—·20
1852·27	3 27	—59	·034	1·89	+·12
1852·72	0 58	+13	·007	1·91	+·04
1853·25	359 35	+ 3	·002	1·83	+·08
1853·78	358 30	—30	·016	1·82	+·05
1854·26	356 10	+16	·008	1·71	+·12

" The signs of the distance-errors would indicate some further slight correction of the elements, but the early distances are not worthy of much confidence.

" *London, 7th June,* 1855."

M. Drach has received a letter from Dr. Donati, of the Florence Observatory, informing him of his having discovered, on the 3d instant, a new comet in the constellation *Telescopium Herschelii.* With the ring micrometer, and a low magnifying power, and through mist and clouds, he obtained the following positions:—

1855.	Florence M.T.	Comet — Star. In R.A.	Comet — Star. In Decl.
	h m s	m s	' "
June 3	10 4 10	—2 17·18	+22 0·0
4	9 55 12	+2 37·78	— 9 22·9
5	9 18 36	—4 2·41	—20 12·5

Comet's App. R.A.	App. Decl.	No. of Comparisons.
h m s	o ' "	
......	......	2 with (a)
6 56 56·27	+36 22 5·5	1 with (b)
7 10 32·73	+36 15 15·1	1 with (c)

Apparent Positions of the Comparison Stars.

	R.A.	Decl.	Catalogue of Stars.
	h m s	o ' "	
(b)	6 54 18·49	+36 31 28·4	Lalande 13569
(c)	7 14 35·14	+36 35 27·6	— 14298

The star (*a*) is of 10.11 magnitude. Clouds prevented its being observed after the evening of the 3d. The comet does not exhibit any trace of either tail or nucleus. It appeared somewhat fainter than the nebula in *Hercules*.

[This comet is the same as that mentioned in the *Monthly Notices* for May as having been discovered by M. Klinkerfues at Göttingen on the 4th of June. Dr. Donati's discovery is, consequently, a day earlier than that of the latter.—EDITOR.]

On the Values of the Constants of Nutation and Aberration, and of the Parallax of γ Draconis, as Deduced from the Observations made with the Twenty-five Foot Zenith Tube, at the Royal Observatory, Greenwich. By the Rev. R. Main.

The observations discussed in this Paper extend from June 1837 to May 1848, during which time they were made with tolerable continuity, the only interruptions being such as were inevitable either on account of periods of bad weather, or from the difficulty of seeing the star in the day-time, or from occasional readjustments or alterations in the instrument.

The author commences with a preliminary recapitulation of the principal results obtained by preceding astronomers for the values of aberration and nutation. He next gives a description of the instrument employed in the observations which form the groundwork of his own investigation, referring for further details to the Introduction of the *Greenwich Observations* for 1837 and subsequent volumes.

The zenith tube was erected by Troughton during the Directorship of Pond for the purpose of observing several stars in addition to *Polaris*, which pass the meridian of Greenwich very near the zenith, but no regular series of observations suitable for any delicate inquiry was made with it till the accession of Mr. Airy. One of the first points taken into consideration by the latter astronomer was the examination and improvement of this instrument, with the view of rendering it efficient for making an extensive series of observations of γ *Draconis*.

It may be mentioned, for the sake of showing the fineness of the micrometer-screw, that the value of a revolution was found from the mean of several determinations made in the winter of 1836 and agreeing very closely with each other to be 6″·714; a similar determination in the spring of 1846 gave 6″·724 for the value of a revolution.

After describing the various precautions which were taken to prevent any disturbing influence from vitiating the observations, the author next proceeds to explain the processes pursued in the calculations.

First, the observations were divided into convenient groups, rarely extending over an interval of time greater than fourteen days ; and it is assumed that for these intervals the mean of the observed apparent zenith distances corresponds accurately to the mean of the days of observation. This supposition is sufficiently accurate even with reference to the changes of the coefficients of aberration and parallax.

The author thus obtains a series of normal apparent zenith distances which theoretically ought to be very accurate, and each of which may be represented by an expression containing the following terms :—

1. The assumed mean north zenith distance for an assumed epoch.
2. The correction due for precession and proper motion for the interval of time comprised between the epoch and the day corresponding to the mean of each group of observed zenith distances.
3. The correction for aberration.
4. The correction for parallax.
5. The correction for nutation.

The assumed epoch is 1840, January 1, and the assumed north zenith distance for that epoch is $118''\cdot00 + v$, the latter quantity representing a correction, the value of which is to be determined from the investigation.

For the precession the assumed value of n is that given by M. Peters in his *Numerus Constans Nutationis* for 1840, namely, $20''\cdot057$; and the assumed proper motion is that deduced by the author himself in his paper on the "Proper Motion of the Fixed Stars." (*Mem. R. Ast. Soc.*, vol. xix.) These, when combined, give $-0''\cdot66$ for the annual variation in north zenith distance. For convenience of application a table is made of the amount of this correction for every hundred days.

The constant of aberration is assumed to be $20'' + x$, and the constant of parallax is represented by y, x and y being unknown corrections.

The constant for nutation is assumed to be for 1840: $9''\cdot223$ $\left(1 + \dfrac{z}{10}\right)$, z being an unknown quantity, which, in like manner as v, x, y, is to be determined from the investigation.

The mean zenith distance corresponding to a group after receiving the various corrections above-mentioned computed for the mean date of the group, is compared with the observed zenith distance, and an equation of condition is thus formed, involving besides the three unknown quantities, x, y, z, a constant and unknown correction to the mean zenith distance denoted by v. A comparison of the computed with the observed normal apparent zenith distance for each group furnishes one of such equations, and thus a series of equations of condition are obtained, from which the values of the unknown quantities v, x, y, z, are to be determined.

The total number of equations of condition is 161.   Treating them by the method of least squares, the author obtains the following results :—

$$v = -0.173; \text{ weight } 528 \cdot 007; \text{ prob. error } 0 \cdot 047$$
$$x = +0.058; \text{ weight } 240 \cdot 192; \text{ prob. error } 0 \cdot 071$$
$$y = -0.378; \text{ weight } 266 \cdot 231; \text{ prob. error } 0 \cdot 067$$
$$z = +0.118; \text{ weight } 289 \cdot 281; \text{ prob. error } 0 \cdot 064$$

Hence, as the assumed mean distance for 1840, January 1, was $118'' \cdot 00$; the constant of aberration $20'' + x$; and the constant of nutation for 1840, $9'' \cdot 224 \left(1 + \dfrac{z}{10}\right)$, we have finally,—

Mean Zenith Distance North of γ *Draconis* for 1840, January 1	$= 117''\!\cdot\!827 \pm 0''\!\cdot\!047$
Constant of Aberration	$20 \cdot 058 \pm 0 \cdot 071$
Constant of Lunar Nutation........................	$9 \cdot 335 \pm 0 \cdot 067$
Constant of Parallax	$-0 \cdot 378 \pm 0 \cdot 067$

The author concludes with the following remarks :—
" The anomalous results here indicated for the aberration and the parallax seem to show that some cause of error of a periodical character has acted injuriously on the observations; but it is very difficult to imagine a source of error of this nature.   A want of fixity of the object-glass, or of the grand micrometer, or of the plumb-line microscopes, would have exhibited itself in producing inconstancy in the zero, and sudden irregularities in the observations; but, on the whole, the zero has, through the whole series of the observations, all desirable steadiness, and the observations are as consistent with each other as could be expected.   The value of a revolution of the grand micrometer was determined by an exceedingly accurate method, twice in the interval of a few years, and the two results were almost identical.   One determination was made in the winter of 1836, and the other in the spring of 1846; and it is probable that the average temperature was pretty nearly the same at each determination; but, even if the result be affected with an unknown temperature correction, the range of arc required for observations of γ *Draconis* is so small, that no significant error is to be apprehended on this account. The only remaining supposition is that of some derangement or deflection of the plumb-line, and the chance of any disturbance of such magnitude and constancy as the case requires having happened, appears to be very small.   If the plumb-line should have been pulled out of the vertical by means of the auxiliary apparatus added in 1840 for the prevention of the twist caused by rapid rotation of the instrument, the deviation would be totally in the prime vertical, and the projection on the meridian plane would still be accurately vertical.   It may be added also, that any deviation of the plumb-line from the vertical would be eliminated by the double observation in reversed positions.

" Still, it is believed that, notwithstanding the doubt which, to a certain extent, seems to rest on the results of the investigation, on account of the anomalous values deduced for the aberration and the parallax, considerable weight may still be attached to the value deduced for the nutation-constant, since, on the supposition of an unknown cause of error producing an inequality, having either an annual or a shorter period, its effect would be perfectly eliminated in so long a series of observations.

" The labour bestowed upon the investigation in the endeavour to produce the best possible result from the observations by applying rigorously the method of least squares, has been very great; and, as the whole of the calculations have been performed by myself, I regret much that the results have not proved so unexceptionable as to warrant the amount of care and labour expended. Still, a laborious work of this kind cannot be without its value; and it is hoped that additional scrutiny may afterwards throw some light upon the nature of the errors in the results of an investigation based upon observations made in an unexceptionable way, with an instrument faultless in theory, and pursued by processes combining all the modern refinements.

" For the present I leave this Paper in the hands of astronomers, with the hope that some one interested in the subject may be able to offer some elucidation of the difficulty thus presented.

" 1855, *May* 30."

Results of Astronomical Observations made at the Observatory of the University, Durham, from October 1849 *to April* 1852, *under the general direction of the Rev. Temple Chevallier, B.D., F.R.A.S., Professor of Mathematics and Astronomy. By* R. C. Carrington, *Esq., B.A., F.R.A.S., Observer in the University.* 8vo. Durham, 1855.

In an Introduction, embracing sixteen pages, the author gives a description of the instruments and mode of observation pursued.

From October 1849 to the beginning of May 1850, the observations were almost exclusively meridional; thenceforward, with little exception, equatoreal. The objects observed in the meridian during 1849 and the spring of 1850 were, for the most part, stars from the Catalogue of the British Association, of which modern observations were wanted, the moon and moon-culminating stars, and the planets *Mars, Jupiter, Saturn, Uranus,* and *Neptune.* After April 1850, the objects were mainly such of the comparison stars used for the equatoreal as were not too faint for the transit telescope. The objects observed with the equatoreal were the planets *Pallas, Juno, Hebe, Iris, Flora, Metis, Hygeia, Parthenope, Victoria, Egeria, Irene,* and *Eunomia;* and the comets called Petersen's third, Bond's, D'Arrest's, Brorsen's fourth, and Encke's.

The equatoreal observations were published provisionally from time to time in the *Monthly Notices* of this Society, and in the *Astronomische Nachrichten.* Their appearance in a definitive form was, however, delayed, in consequence of the transit-circle of the Durham Observatory being too small for observing the greater number of the comparison stars, and the necessity which therefore existed for obtaining those data from some other sources. The author, before his connexion with the university had ceased, offered to provide for this want as far as he was able, and to superintend the future revision and publication of the observations in a final form, as soon as the star-places were supplied. For a part of these he was indebted to the Astronomer Royal and Professor Piazzi Smyth; the others, or at least those which did not require reobservation, were determined by himself at the observatory which he subsequently erected at Redhill. The total number of comparison stars is 221.

The instruments of the observatory consisted of a small transit-circle of $3\frac{1}{4}$ in. aperture, and 4 feet 2 in. focal length; an astronomical clock, by Hardy; an 8-feet equatoreal by Fraunhofer; and a second equatoreal of 7 feet, presented by the late Duke of Northumberland; besides some smaller instruments not in actual use.

The Fraunhofer refractor with which the equatoreal observations were made had an aperture of $6\frac{1}{4}$ in., and a focal length of 8 feet 3 in.

The first section of the work contains a catalogue of the mean places of stars deduced from observations made with the transit-circle. The second section exhibits the apparent positions of planets observed with the same instrument, and their comparison with the corresponding numbers given in the *Nautical Almanac.* The third section is devoted to observations of the moon and moon-culminating stars, also taken with the transit-circle. The fourth section contains the details of occultations of stars by the moon, observed with the Fraunhofer equatoreal, with the equations of condition deduced from them for the correction of the assumed elements computed according to the method of Professor Challis, given in the *Nautical Almanac* for 1854. The total number of occultations treated in this manner is sixteen. In the fifth section the author gives an account of the determination of the longitude of the observatory by the transmission of chronometers between Greenwich and Durham in the year 1851. The chronometers were conveyed twice to and from Greenwich by railway. The value of the longitude deduced from the first journey was 6^m $19^s\cdot79$ west; the corresponding result of the second journey was 6^m $19^s\cdot72$ west. The longitude definitively adopted was 6^m $19^s\cdot75$ west. From observations of the moon and moon-culminating stars, the author's predecessor had found the longitude to be 6^m $19^s\cdot1$ west.

The remaining sections are devoted to the equatoreal observations of the small planets and comets, and to the observations of the comparison stars used in conjunction with them. It is to this part of the work that the author more especially invites attention,

under an impression that the final results may prove of some service in the correction of the orbits of the various bodies to which the observations relate.

On the Determination of the Difference of Longitude of the Royal Observatory, Greenwich, and the Harvard College Observatory, Cambridge, U.S.

(*Extracts of Letters from Mr. Bond of Harvard College Observatory, Cambridge, U.S., to Mr. Hartnup of the Liverpool Observatory.*)

" I have again to solicit your valuable assistance in the further prosecution of the expedition for determining the difference of longitude between this place and Greenwich. The results thus far obtained by means of chronometers prove so satisfactorily accordant that Dr. Bache, the Superintendent of the United States' Coast Survey, deems it expedient to continue our operations. With his permission I enclose you the results of the chronometer expeditions of 1849, 50, and 51, which have been recomputed by my son, Mr. G. P. Bond ; the details are now nearly ready for publication, and will shortly be sent to press.

" The experience of former years has suggested some precautions and minute attentions, not heretofore, I believe, attended to in any similar expedition, which render it desirable to have your co-operation. If you feel disposed, and your other avocations will permit of an affirmative answer, it will afford me great pleasure.

" *March* 14, 1855."

" My son, Richard F. Bond, and Mr. Philip Sidney Coolidge, will go out to Liverpool on the first voyage, and we are very anxious to have everything previously arranged with the authorities of the Liverpool Custom-house, that there may be no delay in landing the chronometers at your observatory immediately on the arrival of the steamer in the Mersey. We have taken state-rooms in the steamer which is to sail from Boston on Wednesday, the 6th of June.

" A spring-governor, and electric clock for records, will accompany the chronometers ; and it will be very desirable to have the temperature maintained as equable and as near that of the mean temperature to which they are exposed on the passage as possible. This being, probably, the last and a test expedition, we shall use every precaution to ensure correct data for the reductions.

" *May* 14, 1855."

Report of the Astronomer Royal to the Board of Visitors, read at the Annual Visitation of the Royal Observatory, Greenwich, 1855, June 2.

The following are a few extracts from this Report:—

" The Twentieth of the Reports which I have had the honour to present to the Board of Visitors will, I trust, be received with indulgence, less on account of the importance of the history which it is to convey, as applying to a year in which no changes of great consequence have been made, than for the numerical place which it occupies in the series of Annual Documents. The date to which the statements of the Report apply is 1855, May 15 ; and the interval embraced in its historical portion is that between 1854, May 26, and 1855, May 15.

" *Astronomical Instruments.*— On the transit-circle, I have only to remark that it is in perfect order, and continues to give the greatest satisfaction to all the observers.

" The transit-circle at the observatory of the Cape of Good Hope (constructed generally on the pattern of that at Greenwich, with some improvements) is mounted, and is, I believe, in full work. It seems to be quite satisfactory to Mr. Maclear.

" The reflex zenith tube is general good order.

" No alteration has been made in the altazimuth.

" The barrel-apparatus, for the register of transits by punctures produced by galvanic communication, has been in constant use without suffering injury, except in the parts exposed to continual friction, which require occasional attention. The method of giving the time-second-signals from the transit-clock, which I I described in my last Report, is found to be perfectly successful. The insulation of the touch-apparatus has sometimes failed in very damp weather; but, when the sky has cleared, the moistened gutta-percha has become dry and the insulation perfect, so speedily that very few transits have been lost.

" The rest of the galvanic apparatus is, in most respects, in the same state as at the last meeting of the Visitors. In the galvanic magnet for dropping the time-signal-ball, it has been found desirable to guard against the risk of permanent magnetism, by causing the apparatus itself to reverse the poles of the battery at every drop of the ball. When arrangements were originally made for exhibiting the London currents upon the transit-clock needle, and for sending currents to and through London by the touch apparatus of the transit-circle, in order to avoid disturbing the ground, I so connected the wires by turn-plates that one of the wires of the barrel-apparatus was used for these purposes; but with the increased facilities which I now possess for laying wires, I intend to make the barrel-apparatus-wires entirely independent of the others,— preserving, however, the power of connecting the touch-apparatus with the London and foreign wires.

" *Astronomical Observations.*— The well-understood system

of meridional observations of stars remains unaltered, each of the stars in the extended standard list being observed, if possible, twenty times in three years. The moon is observed on the meridian at every opportunity, without exception. In the observation of planets a slight relaxation has been made, which the increased number of those bodies rendered necessary. The small planets are not observed at all in the morning watch (from 3^h A.M. to daylight), and the large planets are not observed then except in company with the moon. With this qualification, the sun and planets are observed on the meridian at every opportunity, except on Sundays.

" The whole number of meridional observations from 1854, May 26, to 1855, May 15, is nearly as follows (an observation of two limbs, or a duplicate transit by eye and ear, and by touch-apparatus, being reckoned as two) :—In the transit department; transits, 4680; observations of collimators by means of the transit-telescope, 302; observations of transit-wires by reflexion, 302; observations of one collimator by the other, 52: in the meridian-circle department; observations of all kinds, 4565.

" The number of days on which γ *Draconis* has been observed with the reflex zenith tube is 51.

" With the altazimuth, the number of days of complete observations of the moon during 12 lunations is 178, or 14·8 per lunation, while with the transit-circle the number has been 97, or 8·1 per lunation. The sky has evidently been less favourable than usual; the failing has taken place principally in the spring of the present year. Of the altazimuth observations, o are on days when the moon passed the meridian between 0^h and 1^h solar time, 2 between 1^h and 2^h, 4 between 2^h and 3^h, 3 between 21^h and 22^h, 2 between 22^h and 23^h, 2 between 23^h and 24^h. There are no observations on the meridian corresponding to these. The whole number of separate observations of the moon and stars with the altazimuth is 878, and the whole number of observations of its collimator is 604. The observations have been conducted exactly as in the last years.

" The following remarks on the result of the reductions may not be without interest :—

" During the whole time of which I have spoken, the galvanic-contact method has been employed for transits, with the exception of a few days, when the galvanic apparatus was out of order. From the clock-errors, I have deduced the personal equations of the observers in our usual way; not by making special experiments, in which I have very little confidence, but by taking the transits as we find them, and discussing them on the supposition that the clock-rate has been very steady; a supposition in the adoption of which we are amply justified by the comparison of the clock-errors. The result is, that the magnitude of the personal equations in the galvanic-touch method is not above half of that in the eye-and-ear method.

" In former communications to the Visitors, I have alluded to

the constancy of relation between the azimuths of the transit-circle and of the collimators, while all seem to change with respect to the heavens. Other instances of the same thing have occurred from time to time, and have left upon the minds of the computers the impression that there is a real change in the position of the ground. I need not remark that such a conclusion can be received as valid only after very careful and complete discussion.

" *Chronometers, Communication of Time, and Operations for Longitude.* — The number of chronometers now on hand is about sixty; twenty of these being on trial for purchase. They are compared, some every day and some every week, and occasionally in extreme temperatures; the repairs of those which belong to the Government are managed; and weekly reports of rates, and monthly reports of repairs, are made to the Admiralty.

" The system of galvanic normal clock and sympathetic clocks is in the state described in the last year's Report; with this difference only, that the wires to the clock at the Hospital Schools, instead of being suspended across the Park, are carried underground. The clock at the London Bridge Station is made to distribute the galvanic hourly signals to the Electric Telegraph Company's wires and to the different branches of the South-Eastern Railway wires.

" The time-signal ball at Deal was brought into regular use at the beginning of the present year. In a short time, however, its action was interrupted, partly by derangement of the apparatus, and partly by the severity of the weather, which froze the sulphuric acid to the state of jelly. I sent an assistant and workman to put it in order, and since that time it has generally acted very well. Since March 2 there have been three failures; one of these arose from the ball hanging on the clips, which were not properly oiled, and one from the turning off of the current on the Railway line; the cause of the third has not been traced out with certainty. The success or failure of the drop is known immediately at Greenwich; as the Deal ball, at the termination of its fall, so alters the connexions of wires that a signal is sent to the Observatory. A register is kept in a prescribed form in the Ball-Tower at Deal, and copies of this are sent to the Observatory, leaf by leaf, as soon as they are filled. These cautions I consider to be absolutely necessary for maintaining the regular action of the mechanism under rather difficult circumstances. The whole system is so successful that I have no hesitation in recommending its extension to the Government.

" Application has been made to me from one of the important offices of Government, for the galvanic regulation of their clocks. On considering the risks to which various galvanic communications are liable, and the financial necessity for occupying wires as little as possible, I perceived that it was necessary to devise constructions which should satisfy the following conditions. First, that a current sent once a-day should suffice for adjusting the clock, even if it had gone ten or more seconds wrong. Secondly,

that an occasional failure of the current should not stop the clock.
I have arranged constructions which possess these characters, and
the artist (Mr. C. Shepherd) is now engaged in preparing esti-
mates of the expense. I think it likely that this may prove to be
the beginning of a very extensive system of clock regulation.

" I have commenced examination of observations and prepara-
tion of skeleton forms for the extension of lunar reductions.

· " *General Remarks.* — A fair examination of the statements
already made will show how much our real disposable force has
been weakened, by accidental circumstances, during the last year.
Two Assistants were absent for a month on the Pendulum expe-
dition, and the whole of the extensive calculations which followed
were made in the Observatory. One Assistant was employed for
some time at Deal, another being at the same time partially occu-
pied in London. The longitude of Paris required the absence of
an Assistant for some weeks, and threw a great mass of calculation
upon us. For mounting the model of the transit-circle at the
Exhibition in Paris, one Assistant has been employed in Paris
(with four workmen) for a month. It is under these pressures
that I have been obliged to require the assistance of supernume-
raries for observations—an assistance which I have been com-
pelled to purchase by the sacrifice of part of their time, otherwise
available for computation.

" At the same time, I view the progress of our ordinary work
cheerfully. We have effectually maintained the mastery over it;
and, if no special cause interrupts us, we shall in a few months
have brought everything to the most forward condition in which
it is practicable to keep it.

" For the rest, the general policy of the Astronomical Observ-
atory has been the same as during the last twenty years; to leave
the equatoreal and scrutinizing departments of Astronomy to other
Observatories, but to spare no expense in instruments, no pains in
observation, and no labour in reduction, on standard meridional
observations generally, and on meridional and extrameridional
observations of the moon in particular. And I would fain hope
that the *Greenwich Observations* have assumed such a shape, that
the Astronomer who may desire to find fundamental determina-
tions of the sun's path, of the places of an extensive catalogue of
stars, and of the varying positions of all the moving bodies of the
solar system, presented in the utmost extent and fulness, and
accuracy, may meet with all that he desires in those volumes."

There were exhibited at the meeting of the Society two beau-
tiful engravings,—one representing two views of *Mars* and the
other a view of *Saturn*,—executed from drawings by Captain
Jacob, founded upon his own observations of those planets at
Madras with the Lerebours equatoreal. One of the views of
Mars represents the aspect of the planet on the 18th of March,

1854, at 9ʰ 30ᵐ Madras Mean Time; the other represents its appearance on the 23d of March, 1854, at 6ʰ 54ᵐ. The drawing of *Saturn* refers to the appearance of the planet on the 15th of November, 1852. The transparency of the obscure ring which was first remarked by Captain·Jacob and Mr. Lassell, independently of each other, is very clearly exhibited in this drawing. The drawings of both planets have been engraved at the expense of the Court of Directors of the East India Company, who have liberally distributed copies of them to the Fellows of the Society and other persons interested in the subject of such delineations. A few copies still remain for distribution, which may be obtained by applying at the Apartments of the Society.

Suggestions respecting the Origin of the Rotatory Movements of the Celestial Bodies and the Spiral Forms of the Nebulæ as seen in Lord Rosse's Telescopes. By James Nasmyth, Esq.

" What first set me thinking on this subject was the endeavour to get at the reason why water in a basin acquires a rotatory motion when a portion of it is allowed to escape through a hole in the bottom. Every well-trained philosophical judgment is accustomed to observe illustrations of the most sublime phenomena of creation in the most minute and familiar operations of the Creator's laws, one of the most characteristic features of which consists in the absolute and wonderful integrity maintained in their action whatsoever be the range as to magnitude or distance of the objects on which they operate.

" For instance, the minute particles of dew which whiten the glass-blade in early morn are, in all probability, moulded into spheres by the identical law which gives to the mighty sun its globular form !

" Let us pass from the rotation of water in a basin to the consideration of the particles of a nebulous mass just summoned into existence by the fiat of the Creator, — the law of gravitation coexisting.

" The first moment of the existence of such a nebulous mass would be inaugurated by the election of a centre of gravity, and, instantly after, every particle throughout the entire mass of such nebulæ would tend to and converge towards that centre of gravity.

" Now let us consider what would be the result of this. It appears to me that the inevitable consequence of the convergence of the particles towards the centre of gravity of such a nebulous mass would not only result in the formation of a nucleus, but by reason of the physical impossibility that all the converging particles should arrive at the focus of convergence in directions perfectly radial and diametrically opposite to each other, however slight the 'degree of deviation from the absolute diametrically opposite direction in which the converging particles coalesce at

the focus of attraction, a twisting action would result, and rotation ensue, which, once engendered, be its intensity ever so slight, from that instant forward the nucleus would continue to revolve, and all the particles which its attraction would cause to coalesce with it, would do so in directions tangential to its surface, and not diametrically towards its centre. In due course of time the entire of the remaining nebulous mass would become affected with rotation from the more rapidly moving centre, and would assume what appears to me to be their inherent normal condition, namely, spirality, as the prevailing character of their structure; and as that is *actually* the aspect which may be said to characterise the majority of those marvellous nebulæ, as revealed to us by Lord Rosse's magnificent telescope, I am strongly impressed with the conviction that such reasons as I have assigned have been the cause of their spiral aspect and arrangement.

"And by following up the same train of reasoning, it appears to me that we may catch a glimpse of the primeval cause of the rotation of every body throughout the regions of space, whether they be nebulæ, stars, double stars, or planetary systems.

"The primary cause of rotation which I have endeavoured to describe in the preceding remarks is essentially cosmical, and is the direct and immediate offspring of the action of gravitation on matter in a diffused, nebulous, and, as such, highly mobile condition.

"It will be obvious that in the case of a nebulous mass, whose matter is unequally distributed, that in such a case several sub-centres of gravity would be elected, that is to say, each patch of nebulous matter would have its own centre of gravity; but these in their turn subordinate to that of the common centre of gravity of the whole system, about which all such outlaying parts would revolve. Each of the portions above alluded to would either be attracted by the superior mass, and pass in towards it as a *wisp* of nebulous matter, or else establish perfect individual and distinct rotation within itself, and finally revolve about the great common centre of gravity of the whole.

"Bearing this in mind, and referring to some] of the figures of the marvellous spiral nebulæ which Lord Rosse's telescope has revealed to us, I shall now bring these suggestions to a conclusion. I have avoided expanding them to the extent I feel the subject to be worthy and capable of; but I trust such as I have offered will be sufficient to convey a pretty clear idea of my views on this sublime subject, which I trust may receive the careful consideration its nature entitles it to. Let any one carefully reflect on the reason why water assumes a rotatory motion when a portion of it is permitted to escape from an aperture in the bottom of the circular vessel containing it; if they will do so in the right spirit, I am fain to think they will arrive at the same conclusion as the contemplation of this familiar phenomenon has brought me to.

" Bridgewater Foundry, June 7, 1855."

On Celestial Day Observations.   By Thomas Dick, Esq. LL.D.

" During the last nine or ten years more discoveries have been made in the heavens than at any previous period.   More than thirty planetary bodies, in addition to those formerly known, have been descried by the unwearied observations of European astronomers.   It has frequently occurred to me as not altogether improbable that some new planets might be discovered in the *day-time* in the region of the heavens which lies between the orbit of *Venus* and the sun.

" About the year 1813 I commenced a series of observations on the heavenly bodies in the daytime with a small equatoreal telescope, 20 inches focal length, furnished with powers of 15, 30, 45, and 100 times, with the view of determining the following particulars:— 1.   What stars and planets may be conveniently seen in the daytime, when the sun is above the horizon; 2.   What degrees of magnifying power are requisite for distinguishing them; 3.   How near their conjunction with the sun they may be seen; and, 4.   Whether the diminution of the aperture of the object-glass of the telescope or the increase of magnifying power conduces most to render a star or a planet visible in daylight.   Several hundreds of observations were made in reference to these particulars, and the results were published in vol. xxxvi. of Nicolson's *Philosophical Journal,* for 1813.

" Now, it occurs to me that if a series of observations were made in the daytime on that region of the heavens which lies between the orbit of *Mercury* and the sun, it might be ascertained whether any planetary bodies exist within the orbit of that planet, — a space 37 millions of miles in extent between the orbit of *Mercury* and the central point of the solar system.   But such a body could never be detected in the evening after sunset, as its greatest elongation from the sun could not be supposed to be more than 8 or 10 degrees, and consequently it would descend below the horizon in less than half-an-hour after sunset, and before twilight had disappeared.   The only chance of detecting such a planet would be when it happened to transit the sun's disk; but as this would only happen at distant intervals, and as it might make the transit in cloudy weather, or when the sun is absent from our hemisphere, there is little prospect of our discovering such a body in this way.

" I am of opinion that it is possible, and not at all improbable, that a planet within the orbit of *Mercury* (if such a body exist) might be detected in the daytime, were powerful telescopes applied to a space of the heavens about 12 or 14 degrees around the sun.   Small stars have been seen even at noonday with powerful instruments, and consequently a planet even smaller than *Mercury* might be perceived in the daytime.   On the 2d of October, 1843, about 2 o'clock, P.M., I perceived the planet *Venus,* about 2 hours before her superior conjunction with the sun, when only 58′ from the sun's northern limb.

"In making such observations, it is requisite that a round opaque body be placed at a considerable distance from the observer, so as completely to intercept the body of the sun, and about $1\frac{1}{2}°$ of the heavens all around him; and every portion of the surrounding space, extending to at least 12 or 14 degrees in every direction, should then be carefully and frequently examined by good telescopes. The opaque circular board, or other object, in order effectually to intercept the rays of the sun, should be placed at a considerable distance beyond the object-end of the telescope, and and if any contrivance could be made to make it move along with the sun so much the better, so as to prevent any of the solar rays from entering the tube of the telescope. It would likewise be expedient that the object-glass of the telescope should be somewhat contracted. Such observations, if persevered in, would undoubtedly afford a chance of detecting any revolving body that might exist within such a limit. It is not at all improbable that a planet exists within the range of 36 millions of miles from the sun; and from many observations I have made on *Venus* when very near the sun I am persuaded that a body, though only half the size of *Mercury*, could easily be distinguished in daylight with a good telescope.

"*Broughty Ferry, Dundee, May* 31, 1855."

The King of Prussia has presented a Gold Medal to Lieut. Maury, U.S.N., on account of "the distinguished services which he has rendered to science and navigation by his labours in ascertaining the currents and depths of the ocean, and in determining the direction of the winds at different seasons and in different latitudes." This mark of distinction was also accompanied with the presentation of one of the gold medals struck in honour of the publication of Baron Humboldt's *Cosmos*.

The ephemeris of *Amphitrite* for 1855, published in the supplement to the *Nautical Almanac* for 1858, having been found to exhibit a large difference from observation, Mr. Hind has issued a printed circular containing a portion of M. Villarceau's ephemeris of the planet (*Comptes Rendus*, tom. xl. No. 5), adapted to the meridian of Greenwich. This reprint contains the apparent places of the planet from July 1 to September 30 of the current year.

Remarks on the Construction of Telescopes with Simple Cylindrical Glasses. By M. Sturm.

(*Communicated by Dr. Lee.*)

"I take the liberty of calling attention to the application of cylindrical lenses to the construction of telescopes, which are,

as nearly as is possible, free from the defects of aberration: for the special purpose of grinding and centering these glasses I have constructed and patented a machine, and the glasses so ground and centered appear more perfect than those at present manufactured by hand. The defectiveness of the cylindrical glasses now in use is, I think, the reason why they have not been hitherto more used for scientific purposes. I have made a telescope, by way of experiment, with a cylindrical object-glass ground on my machine; the effect of which sufficiently shows the practicability of my method. Although I have at present but one cylinder in my grinding machine, it is, I think, demonstrable, that when I am enabled to construct the several cylinders of the different focal lengths and diameters, important advantages may accrue to the science; among which might be mentioned,—

"Firstly. Having the object-glass single, thus obviating the necessity of different kinds of glass, and the difficulty of procuring flint-glass of a large size free from imperfections.

"Secondly. Object-glasses could be produced of a diameter of from 20 to 24 inches.

"Thirdly. The magnifying power and the extent of field will be proportionately increased, and with equal precision and exactness.

"Fourthly. The time required for manufacturing such object-glasses is considerably less; and

"Fifthly. The cost is greatly diminished."

It may be as well to remind those who possess telescopes of sufficient optical power for the purpose, that it would be desirable to examine the planet *Venus* on the occasion of its approaching inferior conjunction, with the view of ascertaining whether it affords any indications of a twilight conformably to what has been asserted by some preceding observers. (See *Monthly Notices*, vol. xiv. p. 169.)

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Death of the Rev. Richard Sheepshanks.

It is with deep regret that the Editor has to announce the
heavy loss which the Society has sustained during the recess, in
the death of one of its most valuable and devoted adherents. On
the 4th of August, 1855, the Rev. Richard Sheepshanks, M.A.,
after a short illness, expired at his residence in Reading, aged
sixty-one years. This is not the occasion on which to advert at
length to the admirable qualities of heart which endeared Mr.
Sheepshanks to a wide circle of friends; nor to remark upon the
disinterestedness with which he laboured throughout life in the
cause of science, and more especially upon his untiring vigilance
in promoting the interests of this Society during the long period
of his connexion with it. All this will be recorded in the proper
place. But the Editor may be permitted to state, that in the
death of Mr. Sheepshanks he has to deplore the loss of a warmly-
attached friend, whose valuable counsel and effective aid in dis-
charging the duties of his office were invariably tendered with a
cordiality and kindliness of feeling, the vivid recollection of which
must constitute his apology for alluding to them here.

Observations of the Comet discovered at the Imperial Observatory at Paris on the 4th of June (Comet II., 1855), taken with the $8\frac{1}{2}$-inch Achromatic Refractor of the Liverpool Observatory. By John Hartnup, Esq.

G.M.T.	Comet's R.A.	Log. $\frac{p}{P}$	N.P.D.	Log. $\frac{q}{P}$
1855. d h m s	h m s		° ′ ″	
June 9 11 27 3·5	7 51 51·70	+ 8·582	54 45 35·6	−9·9380
10 10 39 59·4	7 58 40·26	+ 8·641	55 3 0·6	−9·9057
10 11 18 6·1	7 58 51·01	+ 8·597	55 3 26·6	−9·9313

The observations are corrected for refraction. The corrections
to be applied for parallax in time and arc are represented by
p and q. P is the equatoreal horizontal parallax; star of com-
parison, B.A.C. 2860; assumed mean place for 1855, Jan. 0.

$8^h 24^m 3^s·09$ R.A. $53° 4′ 32″·65$ N.P.D.

Observations made in 1851, *to determine, by means of the Electric Telegraph, the Difference of Longitude between Halifax Dock-yard Observatory and Harvard Observatory, Cambridge, Massachussets, U.S.*

(*Communicated by the Astronomer Royal.*)

The observatory in the Naval Yard at Halifax was placed in communication with the observatory at Bangor (Maine), and at Cambridge (Mass.) by means of the electric telegraph.

On the 25th of November, 1851, the break-circuit clock was put in operation at Bangor, and compared with a sidereal chrono-meter at Halifax, but the weather was too cloudy for observations, and continued so until the 16th of December.

The instruments used were four-feet portable transits, by Troughton and Simms, belonging to the United States Coast Sur-vey, having each twenty-five vertical wires arranged in five groups of five wires each, and one horizontal wire through the centre, at the principal focus of the object-glass.

The time was defined by the clock at Bangor breaking circuit every second, which, together with the observations, were recorded on the registers at each place.

The following are the results of observations made on the 16th December, 1851 :—

By Bangor Clock.

		h	m	s
B.A.C. 1287, passed Mⁿ. Halifax, lamp W.	3	43	36·84
— — Camb. — E.	4	13	46·47
Halifax, east of Camb.		30	9·63
B.A.C. 1736, passed Mⁿ. Halifax, lamp E.	5	4	39·87
— — Camb. — W.	5	34	49·49
Halifax, east of Cambridge		30	9·62
Mean free from collimation error ... ...,		30	9·625

	m	s
Bangor clock slow of Halifax by 1414, lamp W. (12 wires) ...	20	26·44
— — — 1476, — (25 wires) ...		26·15
Meaning half effect to No. of wires	20	26·27
Bangor clock slow of Halifax by 1540, lamp E. (25 wires) ...	20	26·60
Meaning lamp E. with lamp W., frees collimation	20	26·435

	m	s
Bangor clock fast of Cambridge by 1364, lamp E. (25 wires) ...	9	43·31
— — — 2429, — — ...		43·54
— — — 2504, — — ...		43·17
Mean, lamp E.	9	43·34

		m s
Bangor clock fast of Cambridge by 2338, lamp W. (25 wires) ...		9 42·79
— — — 2691, — — ...		9 43·21
Mean, lamp W.		9 43·00
Meaning lamp E. with lamp W. frees collimation		9 43·17

	m s
Hence Bangor clock slow of Halifax	20 26·435
— — fast of Cambridge	9 43·170
Halifax, east of Cambridge	30 9·605

By direct observations on B.A.C. 1287 and 1736, Halifax Naval Yard Observatory is east of Cambridge (Harvard Observatory), 30ᵐ 9ˢ·625. By indirect observations, 30ᵐ 9ˢ·605. Taken, 30ᵐ 9ˢ·62.

The transit instrument was 94·7 feet west of Cambridge east transit, that to which the longitude of the observatory is referred.

Therefore Halifax Naval Yard Observatory is 30ᵐ 9ˢ·54 east of Cambridge (Harvard Observatory, east transit).

At this point Professor Bond was taken dangerously ill, and we were unable in consequence to correct again with Cambridge.

Observations were made on the 17th, 19th, 22d, and 23d December, between Halifax and Bangor. The weather was generally unfavourable, and also the state of the telegraph; however, the observations tended to confirm the above result, which cannot be far from the truth.

The mean distance determined by Admiral Owen during 1841, 1844,* and 1845, with chronometers, in which I assisted, was 30ᵐ 9ˢ·9.

	h m s
Harvard East Transit being taken West of Greenwich	4 44 30·54
Halifax, east of Harvard (east transit)	30 9·54
Halifax Observatory west of Greenwich	4 14 21

<div align="right">P. Frederick Shortland,
Commander.</div>

Elements of Comet I., 1855. By M. Winnecke.†

$$T \quad ...... \quad 1855,\ \text{Feb. 5, 76590.}$$

		° ′ ″	
ϖ	......	226 33 4·6	} Equinox 1855,
☊	......	189 40 8·6	Jan. 0.
i	......	51 12·41·2	
Log *q* ...		0·341478	

Motion retrograde.

These elements are calculated from the Moscow observation of April 19, and the Berlin observations of May 6 and May 18.

* In the determination of the longitude of Harvard Observatory the greatest weight has been given to the result of numerous chronometers run between Liverpool Observatory and that at Harvard, by means of the Cunard steamers, in which Liverpool Observatory is taken as 12ᵐ 0ˢ·5 west of Greenwich.

† Discovered by M. Schweizer on the 11th of April; see p. 174, where for Comet II., read Comet I.—Editor.

Discovery of a New Planet.   By M. Luther.

Mr. Hind has received a letter from M. Luther, dated October 8, announcing his discovery of a new planet at Bilk, on the 5th of the same month.   It was situate in $2°\ 25'$ of right ascension, and $0°\ 52'$ of north declination, and resembled a star of the tenth magnitude.   On the following evening he obtained this observation of it :—

	M.T. Berlin.	R.A.	N. Decl.
1855.	h m s	o ′ ″	o ′ ″
Oct. 6.	8 44 14·4	2 12 21·4	+0 49 18·2

This planet has received the designation of *Fides,* with the symbol of a cross.

On certain Anomalies presented by the Binary Star 70 Ophiuchi. By Capt. W. S. Jacob, Madras Astronomer.

The pair of stars designated as 70 *Ophiuchi* have been long recognised by astronomers as a binary system, but the authorities are not as yet agreed as to the exact orbit, although only about $20°$ are wanting to complete the whole ellipse since they were first measured by Sir W. Herschel in 1779.

Many hypothetical orbits have been computed, all of which fail at certain points in representing the observed quantities as closely as might be expected : thus, in some that have lately appeared, while the angles show a tolerably near agreement with observation, the distances are entirely thrown out, the maximum being represented as yet future; whereas, observation indicates a steady decrease in distance since 1848 or 1850, the quantity now amounting to $0''·50$; any attempt, however, to alter the elements so as to bring the distances into better agreement, throws a considerable increase of error on the angles.   There is, further, this remarkable point to be noticed, viz., that even in those orbits in which the distances are neglected, and the angles made to agree as closely as possible, the errors assume somewhat of a periodical or epicyclical form, continuing to have the same sign over a considerable extent of the orbit, and this is still more strongly marked when both angles and distances are taken into account.   Thus, in an orbit which I have lately computed with a period of 93 years, the errors are + from 1820 to 1823, — with one exception from 1823 to 1830 ; from 1830 to 1832 there is a turning point, where the errors alternate ; and from 1833 to 1842, they are again + ; and from 1846 to the present time they are for the most part —.

Now such systematic errors can hardly be casual; they must

depend upon some law. It is just possible that they might arise from an erroneous assumption of the universal application of the law of gravitation; this law may be somewhat modified in these remote systems: the general evidence of observation is in favour of the law being everywhere the same as it is within the solar system, but the data are not exact enough to define this with certainty.

But there is a simpler mode of accounting for the discrepancy without having recourse to any change of law. We may suppose a third body to belong to the system, and to be opaque, and consequently invisible; such a body would, of course, disturb the regularity of the motions of the other two.

The existence of such dark bodies has been already surmised, though not fully demonstrated in the cases of certain apparently single stars, such as *Sirius* and *Procyon.* The body in this case, if it be supposed to circulate as a planet round the smaller star, need not be very large, for the observed amount of deviation of the star from its regular orbit is less than $0''\cdot 1$ of arc.

I have computed the corrections to be applied on the hypothesis of the secondary orbit described by the smaller star having a semi-major axis of $0''\cdot 08$, and an excentricity of $0''\cdot 15$, a periodic time of 26 years, and motion direct, which seems to be somewhat near the truth, for a great improvement is at once manifested; without correction the average error of the angles in the interval from 1820 to 1855 is 49′, which is reduced by the corrections to 35′, or by more than one-fourth, while the maximum error is also reduced from 133′ to 94′. In the distance measures for the same interval the difference is less strongly marked, but the early measures are not worthy of much confidence; and if we consider only the time subsequent to 1838, which includes all the best measures, we find the average error reduced from $0''\cdot 14$ to $0''\cdot 11$, and the maximum from $0''\cdot 26$ to $0''\cdot 19$, being about the same proportion as above.

There is, then, some positive evidence in favour of the existence of a planetary body in connexion with this system, enough for us to pronounce it highly probable, and certainly good ground for watching the pair closely, to procure, if possible, still stronger evidence.

The corrections have not been applied to the three first observations, being those of Sir W. Herschel; these being isolated points, could be easily brought into agreement by a slight change of the elements, and would, therefore, furnish no test of the accuracy of the hypothesis.

Comparison of the Orbit of 70 Ophiuchi, with Observations.

Epoch.	Observed Position Angles.	Computed − Observed.	Error Corrected.*	Observed Distance.	Computed Distance.	Error.	Error Corrected.*
	° ′	′	′	″	″		
1779·77	90 0	+ 5	...	4·49	4·67		
1802·34	336 8	−136	...				
1804·41	318 48	+ 12	...	2·56	2·44		
1820·31	161 27	+133	+65				
1821·51	156 50	+107	+67				
1822·54	154 30	+ 25	+14				
1823·32	153 25	− 50	−41				
1825·56	148 12	− 92	−44	4·38	4·29		
1826·75	146 24	−133	−80				
1827·40	143 54	− 64	−10	4·51	4·64		
1828·67	140 18	+ 13	+67	4·79	4·87		
1829·50	139 30	− 23	+23	5·18	5·00		
1830·36	138 9	− 22	+ 8				
1830·50	137 28	+ 6	+35	5·65	5·20		
1830·76	136 24	+ 46	+71	5·43	5·24		
1831·55	136 8	− 5	+ 8	5·97	5·37		
1832·55	133 46	+ 53	+53	5·73	5·52		
1832·57	135 31	− 54	−54	5·50	5·52		
1833·42	132 49	+ 43	+33	6·14	5·65		
1833·59	132 30	+ 49	+37	5·98	5·68		
1835·56	130 36	+ 17	−13	5·97	5·91		
1836·81	128 36	+ 52	+12	6·33	6·05		
1837·64	127 30	+ 63	+19	6·40	6·14	″	″
1838·51	126 30	+ 67	+20	6·25	6·22	−·03	−·05
1842·55	122 24	+ 67	+27	6·68	6·57	−·11	−·06
1846·21	120 10	− 2	− 2	6·83	6·64	−·19	−·11
1848·12	118 50	− 27	−10	6·80	6·66	−·14	−·06
1850·48	115 11	+ 64	+94	6·86	6·65	−·21	−·17
·66	117 0	− 55	−23	6·50	6·64	+·14	+·16
1852·75	114 3	+ 5	+40	6·73	6·61	−·12	−·13
1853·60	114 39	− 77	−46	6·49	6·58	+·09	+·05
1854·08	113 39	− 44	−16	6·36	6·57	+·21	+·16
1854·24	113 2	− 16	+10	6·51	6·57	+·06	+·01
1854·73	113 43	− 84	−62	6·34	6·55	+·21	+·16
1855·45	111 35	+ 4	+21	6·26	6·52	+·26	+·19
		49	·35			·14	·11

Elements of the Orbit of 70 Ophiuchi.

$$\tau = 1808^{r}·12$$
$$\varpi = 292° 32′$$
$$\Omega = 304·32$$
$$\lambda = -20\ 28$$
$$e = ·4894$$
$$\gamma = 55° 16′ \qquad \cos = [9·7557]$$
$$P = 93^{r}·10$$
$$n = 3°·867$$

* On the hypothesis of a secondary orbit, in which $a = 0″·08$, $e = 0·15$. $\varpi = 200°$, $P = 26^{yr}$, $r = 1825·5$.

*Elements of Comet II., 1855. By M. Bruhns.**

$$T \quad \text{......} \quad \text{1855, May 29, 23896, M. T. Berlin.}$$

$$\varpi \quad \text{......} \quad \overset{\circ}{240} \; \overset{\prime}{15} \; \overset{\prime\prime}{18\cdot4} \left.\right\} \; \text{Mean Equinox}$$

$$\Omega \quad \text{......} \quad 260 \; 52 \; 43\cdot1 \left.\right\} \quad 1855, 0.$$

$$i \quad \text{......} \quad 22 \; 58 \; 27\cdot1$$

$$\text{Log } q \text{...} \quad 9\cdot745678$$

Motion retrograde.

These elements were calculated from the Berlin observations of June 5, 6, and 7.

Discovery of a New Planet. By M. Goldschmidt.

On the 5th of October M. Goldschmidt discovered a new planet at Paris; it resembled a star of the 11–12th magnitude. The following observation of its right ascension was taken at the Imperial Observatory, Paris : —

1855.	M.T. Paris.	R.A.
	h m s	h m s
Oct. 7	9 55 37·9	22 59 31·86

The declination is wanting.†
This planet has received the name of *Atalanta*.

Observations of the two New Planets discovered by M. Goldschmidt and M. Luther on October 5, 1855, made with the Hamburg Equatoreal. By Mr. G. Rümker.

(*Communicated by Dr. Lee.*)

M. Goldschmidt's New Planet (*Atalanta*).

1855.	Hamb. M.T.	App. R.A.	Decl.	App. Place of Star of Comp.
	h m s	o ′ ″	o ′ ″	h m s o ′ ″
Oct. 12	10 13 53	343 50 38·7	−6 48 42·3 (5 comp.)	22 56 59·44 −6 57 19·3
13	8 4 36	343 40 42·6	−6 40 34·5 (18 comp.)	22 55 33·91 −6 36 53·8

M. Luther's New Planet (*Fides*).

1855.	Hamb. M.T.	App. R.A.	Decl.	App. Place of Star of Comp.
	h m s	o ′ ″	o ′ ″	h m s o ′ ″
Oct. 7	7 50 57	2 0 3·3	+0 45 49·0 (17 comp.)	0 6 25 21 +0 34 59·3
8	7 46 54	1 47 14·9	+0 41 55·1 (15 comp.)	{ 0 6 25 21 +0 34 59·3 ; 0 7 14 00 +0 29 45·9
13	10 10 13	0 44 57·0	+0 23 59·4 (19 comp.)	0 2 4 94 0 26 30·3

* This is the comet alluded to at p. 203, as having been discovered by M. Klinkerfues on the 4th of June. It was also discovered independently on the same evening at the Imperial Observatory of Paris, by M. Dien, and on the preceding evening by Dr. Donati at Florence.— EDITOR.

† It appears to be about − 6½°, see *infrà*.—EDITOR.

Supplement to the Nautical Almanac for the year 1859.

This Supplement (which has just been published previous to
the appearance of the Almanac itself) contains Ephemerides for
the year 1856, adapted to the meridian of Greenwich, of *Ceres,
Pallas, Juno,* and *Vesta;* approximate at intervals of four days,
and accurate at each transit near the times of their respective
oppositions; with the elements from which they have been de-
duced. Also, approximate Ephemerides of all the newly-discovered
Planets, with the exception of *Circe, Leucothea,* and the two just
announced, one by Dr. Luther, and the other by Mr. Goldschmidt;
with elements in a few special cases of addition of perturbations.

It may be proper to remark with respect to *Ceres* and *Juno,*
that the necessity for a correction of their elements has been for
some time apparent; but that as regards *Pallas* and *Vesta,* the
case is less urgent, hence the reason for allowing the two latter to
continue uncorrected for the present.

A letter has been received from Mr. Mayne, a gentleman
residing in Killaloe, county of Clare, Ireland, in which he states,
with reference to an extract of a letter from M. Sturm on the
construction of cylindrical object-glasses, which was communi-
cated to the Society by Dr. Lee, and inserted in the *Monthly
Notices* for last June, that he purchased glasses of a similar con-
struction in Paris as early as the year 1814. Mr. Mayne says
nothing respecting the *practical value* of those glasses as regards
their achromatic and other qualities ; he, no doubt, presumes that
any information on this point would be unnecessary. The Editor
avails himself of this opportunity of repeating what has been
already announced on several previous occasions — that the
Society distinctly disavows any responsibility in connexion with
the opinions expressed in any of the papers which appear in its
publications.

The Editor is indebted to the Astronomer Royal for pointing
out (in a letter dated Aug. 11, 1855) an oversight committed in a
note inserted in the *Monthly Notices* for last June (p. 224), relative
to the expediency of observing *Venus* on the occasion of her ap-
proaching inferior conjunction in October. " There is no possi-
bility," says Mr. Airy, " of seeing *Venus* near this conjunction:
1st, because, even if she were in the ecliptic, the ecliptic itself is
so nearly parallel to the horizon, that nothing can be seen within
30° of the sun (this, however, does not apply to the rising);
2dly, because she has a large southern latitude. The only con-
junctions when *Venus* can be advantageously seen are those in
the spring."

LIST OF PRESENTS

RECEIVED DURING THE SESSION OF 1854-55,

AND OF

BOOKS PURCHASED WITH THE TURNOR FUND

DURING THE SAME PERIOD,

FORMING

APPENDIX VI.

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ERRATA, VOL. XV.

Page 12, line 8 from bottom, *for* a large number, *read* a moderate number.
— 14, — 1, *for* were different, *read* were not different.
— — last line, *for* greater than unity, *read* less than unity.
— 49, line 2 from top, *for* Lieut. G. M. Gilliss, *read* Lieut. J. M. Gilliss.
— 159, — 11, *for* Comet I., 1855, *read* Comet V., 1854.
— 161, — 3 from bottom, *for* agreement, *read* argument.
— 174, — 10, *for* Comet II., 1855, *read* Comet I., 1855.
— 179, the upper line of Fig. X should be slightly curvilinear, so as to adapt itself to the periphery of the elliptical orbit of the star.
— 181, line 4 from bottom, *for* $52° 24' 52''$, *read* $52° 54' 52''$.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1855, TO JULY 1856.

<hr>

VOL. XVI.

BEING THE ANNUAL HALF-VOLUME OF THE MEMOIRS AND PROCEEDINGS
OF THE ROYAL ASTRONOMICAL SOCIETY.

<hr>

M. J. JOHNSON, Esq. President, in the Chair.

John Cockle, Esq. M.D., 107 Guildford Street, Russell Square,
was balloted for and duly elected a Fellow of the Society.

Account of Recent Astronomical Operations in Russia.

(*Extract of a Letter from M. Otto Struve to the Astronomer Royal, dated
Oct. 21, 1855.*)

" When I last wrote you, I said that galvanic telegraphy was
quite in its infancy in Russia. Since then the war has prompted
that affair in a rapid manner. At this moment we have already in
Russia about 6000 miles, or even more, of galvanic wires, and
are on one side through Warsaw and Cracow; on the other side,
through Königsberg, in connexion with the foreign lines. But to
make use of these lines for scientific purposes will hardly be
possible before the close of the war, for at present all the lines are
continually used for official despatches. Only one short line has
served for scientific objects, — this is the line of Petersburgh to
Cronstadt, by which I have to transmit regularly exact Pulkowa
time to that part for the purpose of regulating the rates of the
chronometers of our navy. This is a small part of the duties
devolved upon me by a new appointment as Consultative Astro-
nomer to the Admiralty, in the same manner as I was already
since 1848 engaged with the Imperial General Staff. By this
supplementary appointment, the geographical part of my sphere of
activity has considerably increased, and consequently I am yet more
limited than before in the pure scientific astronomical pursuits.

" It is really remarkable that the war until now has not ex-
ercised the least influence on the progress of any scientific pur-
suit for which the support of government is wanted. On the con-
trary, the energy elicited by the state of war in one principal
direction has given rise also to a development of energy in many
other respects. This will be proved in part by a short enumera-
tion of the principal geographical undertakings, in the arrange-
ment or direction of which we had to take a part this year. First
started from here a numerous party, under the direction of Mr.
Schwarz, for the exploration of Eastern Siberia; another party
was sent to the Steppes of the Kirghis; a third, under personal
direction of Döllen, had to fix the exact geographical positions of
a large number of points situated in or near the Ural Mountains,
to form a base for the construction of an exact topographical map
of the vast districts of mines in that part of Russia; a fourth

expedition, provided with forty chronometers, has to join, first, Moscow with Saratow ; and then, this latter town with Astrachan ; and, finally, the great trigonometrical operations in the southern part of Russia and in the Transcaucasian provinces are carried on without the least interruption. From the last-mentioned circumstance you will conclude that on our part both the astronomical and the geodetical part of a great arc of parallel will be finished in a very short time.

"In my astronomical pursuits the parallaxes of fixed stars have taken a prominent part during the last year, and I think I have made a considerable progress in these researches. Now that the methods of observations are entirely fixed, I am quite sure that if there is a difference of parallax of $0''\cdot 1$ between any couple of stars situated at a distance less than $5'$ from another, four observations made at the epochs of maxima and minima will be entirely sufficient to prove its existence and to define its amount within very narrow limits. I have not yet calculated exactly other parallaxes besides those of α *Lyræ* and 61 *Cygni*, the general results of which are known to you, but a short review of my observations shows that μ *Cassiopeiæ* has a parallax of more than $0''\cdot 3$, η *Cassiopeiæ* of more than $0''\cdot 2$, and *Capella* of between $0''\cdot 1$ and $0''\cdot 2$. For all these cases, the results obtained by the angles of position agree remarkably with those furnished by the distances.

"The observations of other stars, namely, of α *Tauri*, α *Aquilæ*, α *Andromedæ*, and α *Cassiopeiæ*, are about to be closed ; but to guard me against any preoccupation, not even the first step has been made for the reduction of these observations."

The Positions of 20 *Polar Stars, as determined at Redhill.* By R. C. Carrington, Esq.

In the belief that the publication of the positions of an increased number of polar stars suitable for the determination of the meridian error and polar-point circle-reading of a meridian instrument, may be of general utility, I somewhat anticipate the future appearance of the catalogue I have in progress, and give at once the values I now possess, and am satisfactorily applying at Redhill.

The places here given are the result of a careful discussion of 612 observations taken (simultaneously in each element) between the months of December 1853 and April 1855 inclusive. Of these, the older established stars α, ϑ and λ *Ursæ Minoris*, and 51 *Cephei*, appropriate 326, leaving 286 observations to the remaining 16, the numbers of observations of which individually range from 9 to 28. The constants used in their reduction are those given by Peters in his *Num. Const. Nut.* Terms of the second order were omitted, but the small terms which have for argument twice the moon's longitude, were applied throughout. The epoch is the instant at which the sun's mean longitude was 280° near the beginning of the year 1855.

	R.A.	N.P.D.		R.A.	N.P.D.
	h m s	o ′ ″		h m s	o ′ ″
...	......	......	12	12 14 22·10	1 29 46·8
I	0 45 30·70	1 45 23·9	...	......	......
Pol.	1 6 31·20	1 27 48·8	...	......	......
...	......	......	13	13 12 10·50	1 34 26·2
3	2 30 34·50	2 2 53·0	...	......	......
...	......	......	14	15 25 59·70	2 13 9·4
4	3 52 25·10	4 50 7·5	...	......	......
...	......	......	15	16 6 34·95	4 17 20·0
5	5 15 59·15	4 53 35·0	...	......	......
6	5 48 0·40	3 14 26·6	...	......	......
...	......	......	☽	18 19 6·85	3 24 0·6
51 C	6 31 6·60	2 44 51·3	...	......	......
8	7 3 39·60	0 58 10·3	...	......	......
...	......	......	λ	20 8 29·90	1 7 27·4
...	......	......	18	21 27 40·75	3 34 15·9
9	9 47 45·20	2 0 37·5	...	......	......
...	......	......	19	22 30 46·20	2 39 23·7
10	10 52 57·50	1 34 29·3	...	......	......
...	......	......	20	23 27 49·50	3 29 32·4
N	11 57 21·80	3 36 32·5	...	......	......

It would too much increase the length of this communication for me to explain fully the process of discussion which has led to the positions at present adopted, but one phenomenon of observation presented itself in the course of it which I am desirous to state, as it may possibly prove to be the discovery of a source of discordance not hitherto noticed. The circumstance to which I allude is, that observations of those polar stars which are visible in daylight require (in my own case, at least), when observed in daylight, a sensible correction in both elements before they are comparable with observations taken by night, and consequently that an azimuth error, or a latitude deduced from observations of a star alternately above and below the pole, not so corrected, will generally be in error.

It is my practice, in reducing transits, to use the formula

$$c \cdot \frac{1}{15} \cdot \operatorname{cosec} \delta + m \cdot \frac{1}{15} \cdot \cotan \delta + n$$

The collimators provide the means of always keeping the collimation error c so small, that for the region I am working upon the residual amount may be neglected as collimation error, and merged with m the meridian error, given by observation; the constant n being lumped with clock-error.

Now when the separate values of m and of the polar-point circle-reading deduced from each star were arranged, as in the table for the latter part of August 1855, given below, it immediately became apparent, after a first approximation to the finally adopted mean positions, that the corrections I have mentioned would be required, and that they held good with a degree of consistent uniformity which could hardly have been expected.

The mean of 36 daylight observations of *Polaris* gave an

excess over night values of — 1″·3 in *m,* and of + 1″·4 in *p.*   For
☽ *Urs. Min.* 24 obs. gave — 0″·8 in *m,* and + 0″·9 in *p.*   For
51 *Ceph.* 19 obs. gave — 0″·9 in *m,* and + 0″·9 in *p.*   The dis-
crepant signs among the values were very few.

In practice I consequently now apply to all values of *m* and *p*
deduced from daylight observations of *Polaris,* the corrections
+ 1″·3 and — 1″·3 respectively, and for ☽ *Urs. Min.* and 51 *Ceph.*
+ 0″·9 and — 0″·9.

We may, perhaps, have some difficulty in deciding between two
possible sources of this discordance.   Either we may suspect that
the excess in *m* indicates that the instrument has a diurnal varia-
tion of position arising from the action of temperature, and that
the excess in *p* indicates the necessity of some correction to the
refraction-table used ; or, secondly, we may suspect that both
excesses arise from difference of optical circumstances.

In the case of my own transit-circle the annual range of *m*
appears, from the exact observations of two years, to be about 9
seconds, the greatest + values being reached at the beginning of
March, and the greatest — values at the end of August ; and so
far the signs would agree with the idea of the difference arising
from variation of temperature.   But, on the other hand, the
amount of these excesses of *m* derived from observations made at
very different hours in the day, and at times when the diurnal
range of temperature, has been very different, will not support the
notion.   Further, an inspection of the series of values of *m* de-
duced from frequently as many as 6, 8, and 10 stars in one night,
during which the thermometer will have progressively fallen 10°
or 15°, does not appear either to support the notion, there being as
many nights exhibiting no progression of value as of those which
may, perhaps, be thought to do so.

I am accordingly inclined, till I have better grounds for form-
ing an opinion, to lean to the second of the possible causes before
named, partly from one hypothesis in that case meeting both dis-
cordances, partly because I think this view rather supported by
the brighter star *Polaris* requiring so sensibly different corrections
from the fainter ones, and partly because small discrepancies thus
originated are not altogether unknown to us, as might be illus-
trated by the history of the restoration of the standard yard.

Should this view be supported hereafter, we may, perhaps,
find these corrections added to the list of personal equations.

I somewhat regret that the observations in which I am now
engaged do not admit at present of my making such new arrange-
ments as would facilitate the immediate settlement of this doubtful
point, and that I have to content myself with recommending it to
the consideration of those who are engaged in exact meridional
astronomy.

I shall be glad if the present statement should lead any other
observer to communicate anything from his experience which may
elucidate the subject.

In the following table, which is added partly in explanation of

a previous allusion, and partly to give confidence in the use of the published positions of the 20 stars, the asterisk against a star's number indicates that it was observed below the pole ; the column *w* shows the number of wires over which the star was observed in R.A.; the column *t* gives the seconds resulting from time of passing mean wire + correction for clock-error + reduction to 1855·0; the column *f* contains the factors $\frac{1}{15}$ · cotan δ; the column *d* gives the number of seconds by which the seconds in column *t* are less than the adopted right ascensions; the column *m* contains the separate values of the corrections for meridian error found by dividing *d* by *f* for each star. The column *p* contains, in like manner, the seconds of the circle-reading of the polar-point given by each star. These circle-readings result from the use of a single pair of microscopes, usually read by Mr. Simmonds, but read on the 26th and 31st of August by Mr. Carrington.

1855.	Star.	w.	t.	f.	d.	m.	Apply.	p.	Apply.
			s		s	′ ″	° ′ ″		
Aug. 21	☾	5	13·56	+ 1·123	− 6·71	−6·0	359 56	54·9	
	51 C*	5	58·84	−1·388	+ 7·76	−5·6	———	54·1	
	8*	3	17·09	−3·930	+22·51	−5·8		53·5	
	λ	3	51·84	+ 3·402	−2r·94	−6·5		53·4	
	11*	5	15·85	−1·057	+ 5·95	−5·6	−5·9	54·6	54·1
26	8*	3	18·61	− 3·929	+20·99	−5·4		54·4	
	λ	5	47·83	+ 3·405	−17·93	−5·3		53·2	
	9*	3	33·77	−1·897	+11·43	−6·0		54·1	
	19	3	54·89	+ 1·437	− 8·69	−6·0		54·4	
	10*	1	46·62	−2·424	+10·88	−4·5	−5·5	54·8	54·3
28	☾	5	13·08	+ 1·123	− 6·23	−5·6		55·3	
	·51 C*	5	59·36	−1·388	+ 7·24	−5·2		55·0	
	8*	3	21·28	−3·929	+18·32	−4·7		54·6	
	λ	5	46·76	+ 3·405	−16·86	−4·9		54·5	
	20	4	55·77	+ 1·093	− 6·27	−5·8	−5·2	54·3	54·7
29	8*	3	22·66	−3·929	+16·94	−4·3		55·0	
	λ	5	45·14	+ 3·405	−15·24	−4·5		54·1	
	20	2	55·90	+ 1·093	− 6·40	−5·9		54·8	
	11*	5	17·23	−1·057	+ 4·57	−4·3	−4·6	54·3	54·6
30	8*	3	21·93	−3·929	+17·67	−4·5		55·2	
	λ	5	44·16	+3·405	−14·26	−4·2		55·7	
	9*	5	34·69	−1·897	+10·51	−5·6		54·4	
	12*	1	12·04	−2·552	+10·06	−4·0	−4·7	55·1	55·0
31	8*	3	26·81	−3·929	+12·79	−3·3		54·8	
	λ	5	42·25	+ 3·405	−12·35	−3·6		54·1	
	18	3	45·92	+ 1·070	− 5·15	−4·8		54·7	
	11*	2	17·83	−1·057	+ 3·97	−3·8	−4·0	54·1	54·5

Note.—A comparison of the mean errors of single values of *m*, given by stars of different polar distance, does not lead to a preference of one star over another on account of its polar distance, within the limits of those contained in the present list.

Remarks upon certain Cases of Personal Equation which appear
to have hitherto escaped notice, accompanied with a Table of
Results. By the Astronomer Royal.

" My valued friend, Mr. Sheepshanks, in the course of his
micrometrical comparisons of standards à *traits,* discovered (what I
believe had never before been suspected,) that when the defining-
lines of a standard of length are placed under two micrometer-
microscopes, and the moveable wires of the micrometer are made
to coincide with the images of these lines, different observers place
the micrometer-wires in different positions; and that the dif-
ference is not the same with different standard-bars; so that in
the micrometrical comparisons of two standards à *traits* there is a
personal equation peculiar to each observer.   Although the origin
of this is very obscure, yet the circumstance that different micro-
scopes are employed for the two ends seems to leave an opening
for an explanation : although I am not in any way prepared to say
how it can sufficiently account for the discordance observed.

" Mr. Dunkin, however, has lately pointed out to me a case of
personal equation which appears more difficult to explain, where
the same microscope is used for observing both divisions, namely,
in measuring the interval between two adjacent divisions of a
graduated circle for the ordinary correction for runs.   The phe-
nomenon is so singular that I think it may be worthy of the
attention of the Society.

" Mr. Dunkin first discovered this peculiarity in the estimation
of the correction for runs of the horizontal circle of the Alta-
zimuth.   As the illuminating light is carried there in the ob-
server's hand, I thought it possible that the mode of holding the
lamp might account for the difference.   I therefore requested Mr.
Dunkin to collect in a digested form the observations for the
Transit Circle in which the illuminators are fixed, and in which
the magnifying power of the microscopes is so great as to give
considerable certainty on the instrumental results.   These obser-
vations (at least their first results) are contained in the table which
I now lay before the Society.   The number exhibited is the cor-
rection for a measure of 100″: it ought to be multiplied by 3 and
to have its sign changed, in order to give the apparent excess of
the space of 5′ on the limb above a certain definite measure in the
micrometer-microscope.   The observers are Mr. Henry, Mr. Dun-
kin, Mr. Henderson, Mr. Ellis, Mr. Todd, and Mr. Criswick : all
acute and experienced observers.   The observations are so inter-
mixed, in regard of time, that no peculiarity of season or other
circumstance that I can discover will account for the difference.

" It will be seen that in each of three years the correction on
100″ found by Mr. Henry exceeds that found by Mr. Dunkin by
about one-eighth of a second.   The correction found by Mr. Ellis
was at first nearly the same as that of Mr. Dunkin; but in later
years Mr. Ellis's numbers have approximated more to Mr. Henry's.

"I subjoin the table, containing the whole of the individual corrections for Runs, from which these conclusions are drawn.

G. B. AIRY.

"*Royal Observatory, Greenwich,*
"1855, *November* 21.

Comparison of the Observations for Correction for Runs of Micro scopes of Transit Circle, arranged according to observers.

Month and Day.	Correction for 100″ deduced from the Observations for Correction for Runs of Microscopes of Transit Circle.			
	H.	D.	J. H.	E.
1853.				
Jan. 3	... ...	+0·288		
10	... ...	... ...	+0·347	
17	+0·381			
31	... ...	0·246		
Feb. 7	... ...	... ...	0·350	
14	0·323			
21				
28	... ...	0·246		
March 7	... ...	... ...	0·534	
14	0·427			
21				
28				
April 4				
11	0·340			
18				
25	... ...	0·322		
May 2	... ...	... ...	0·473	
9	0·301			
16	... ...	0·280		
23	... ...	... ...	0·427	
30	0·479			
June 6	... ...	... ...	... ...	+0·246
13	... ...	0·340		
20	... ...	... ...	0·288	
27	0·483			
July 4	... ...	... ...	... ...	0·187
11	... ...	0·361		
18	... ...	... ...	0·357.	
25	0·503			
Aug. 1	... ...	... ...	... ...	0·316
8	... ...	0·253		
15	... ...	... ...	0·587	
22	... ...	... ...	... ...	0·378
29	0·448			
Sept. 5	... ...	... ...	... ...	0·295
12	... ...	0·472		
19	... ...	... ...	0·427	
26	... ...	... ...	... ...	0·385
Oct. 3	0·475			

Month and Day.	Correction for 100″ deduced from the Observations for Correction for Runs of Microscopes of Transit Circle.			
	H.	D.	J. H.	E.
1853.				
Oct. 10	... ...	+0·340		
17	... ...	... ...	+0·417	
24	... ...	... ...	... ...	+0·315
31	+0·406			
Nov. 7	... ...	0·284		
14	... ...	... ...	0·406	
21	... ...	... ...	... ...	0·240
28	0·381			
Dec. 5	0·510			
12	... ...	0·288		
19	... ...	... ...	0·323	
28	... ...	... ...	....	0·350
Mean ...	+0·420	+0·310	+0·411	+0·301
1854.				
Jan. 2	0·132			
9	... ...	0·184		
16		... ...	0·305	
23		... ...	... ...	0·360
30	0·333			
Feb. 6	... ...	0·277		
13	... ...	... ...	0·343	
20	...· ...	... ...	... ...	0·350
27	0·264			
March 6	... ...	0·354		
13	... ...	... ...	0·406	
21	... ...	... ...	... ...	0·253
27	0·336			
April 10	... ...	... ...	0·455	
17	... ...	... ...	... ...	0·295
24	0·455			
May 1	... ...	0·385		
8	... ..	... ...	... ...	0·486
15	... ...	... ...	... ...	0·465
29	0·284			
June 5	... ...	... ...	... ...	0·500
12	....	0·291		
19	... ...	... ...	... ...	0·350
26	... ...	... ...	... ...	0·357

Month and Day.	Correction for 100″ deduced from the Observations for Correction for Runs of Microscopes of Transit Circle.			
	H.	D.	E.	T.
1854.				
July 4	... ...	... ...	... ...	+0·263
10	... ...	... ...	+0·489	
17	+0·514			
24	... ...	+0·343		
31	... ...	... ...	... ...	0·378
Aug. 7	... ...	... ...	0·448	
14	0·611			
21	... ...	0·284		
28	... ...	... ...	... ...	0·250
Sept. 4	0·442			
11	... ...	... ...	0·361	
18	... ...	0·264		
25	0·517			
Oct. 2	... ...	... ...	... ...	0·395
9	0·497			
16	... ...	... ...	... ...	0·424
23	0·455			
30	... ...	... ...	0·372	
Nov. 6	... ...	... ...	... ...	0·319
13	0·399			
20	... ...	... ...	0·222	
27	... ...	0·177		
Dec. 4	... ...	... ...	... ...	0·257
11	... ...	... ...	0·350	
18	... ...	0·177		
27	... ...	... ...	0·222	
Mean ...	+0·403	+0·274	+0·368	+0·329
1855.				
Jan. 1	0·486			
8	0·392			
15	... ...	... ...	... ...	0·274
22	... ...	0·167		
29	... ...	... ...	0·264	
Feb. 5	0·157			
12	... ...	0·135		
19	0·194			
26	... ...	... ...	0·347	
March 5	0·428			

Month and Day.	Correction for 100″ deduced from the Observations for Corrections for Runs of Microscopes of Transit Circle.			
	H.	D.	E.	C.
1855.				
March 12	... ...	+0·108		
19	+0·455			
26	... ...	... ...	... ...	+0·507
April 2	... ...	... ...	... ...	0·236
9	... ...	... ...	+0·465	
16	0·395	... ...		
23	... ...	... ...	... ...	0·326
30	.. ...	... ...	0·347	
May 7	0·500			
14	... ...	0·347	... ...	0·240
28	... ...	..., ...	0·271	
June 4	0·663			
11	... ...	... ...	... ...	0·316
18	... ...	0·323		
25	... ...	... ...	0·412	
July 2	... ...	... ...	... ...	0·170
9	0·431			
16	... ...	... ...	0·378	
30	... ...	0·458		
Aug. 6	... ...	... ...	... ...	0·204
13	... ...	... ...	0·513	
20	0·497			
27	... ...	0·497		
Sept. 7	... ...	... ...	... ...	0·402
10	... ...	... ...	0·462	
17	0·445			
24	... ...	0·340		
Oct. 1	... ...	... ...	... ...	0·243
8	... ...	... ...	0·472	
15	... ...	0·298		
22	... ...	... ...	... ...	0·319
30	... ...	... ...	0·562	
Nov. 7	... ...	... ...	... ...	0·229
Mean ...	+0·420	+0·297	+0·408	+0·290

Assemblage of Means.

	H.	D.	J. H.	E.	T.	C.
1853	0·420	0·310	0·411	0·301		
1854	0·403	0·274	... ...	0·368	0·329	
1855	0·420	0·297	... ...	0·408	... ...	0·290

On the Theory of Astronomical Refractions.
By Sir John W. Lubbock, Bart. F.R.S.[*]

In the year 1840 the author of this paper investigated the subject of Astronomical Refractions upon a different hypothesis of the constitution of the atmosphere from that assumed by preceding inquirers, and instituted a comparison between the results and the corresponding refractions inserted in the *Connaissance des Temps*, as well as those of Ivory and Bessel. The present communication, besides embodying the substance of his researches on that occasion, contains also some additional remarks and comparisons tending to elucidate more fully his previous labours.

The following table exhibits the constitution of the atmosphere, which the author was induced to adopt as the basis of his investigation :—

Height in Miles.	Pressure p. Inches.	Temperature τ. Fahr.	Density ϱ.
0	30·00	+ 50·0	1·00000
1	24·61	35·0	·84611
2	20·07	19·5	·71294
3	16·25	+ 3·4	·59798
4	13·06	− 13·3	·49903
5	10·41	30·6	·41403
10	2·81	126·4	·14499
15	·45	240·6	·03573
22·35	...	−448·0	...

We extract the following remarks :—

"Ivory, in his paper on refractions published in the *Philosophical Transactions* for 1838, instituted a comparison between the result of his theory and Bessel's table; and in p. 224 of that paper a table is given offering a comparison between the table of the *Conn. des Temps*, his own, and Bessel's. When afterwards, in the year 1840, I endeavoured to give a solution of this problem, assuming an atmosphere differently constituted from that of Ivory, and, in my opinion, nearer the truth, I also published the table, in p. 133, giving a similar comparison, and I took for Bessel's refractions those given as such by Ivory. It was not until very recently that I perceived that Ivory had altered all Bessel's figures by adding to the logarithm of the refraction according to Bessel, a constant quantity. As Ivory's description of the nature of this alteration appears to me to be somewhat obscure, I applied to Mr. Adams for his opinion, and he writes to me thus : 'Ivory's object, in his comparative table of refractions, is to compare the *laws of variation* of the refraction corresponding to different theories as to the constitution of the atmosphere, and not the *absolute amount* of the refraction, which will, of course, depend upon the assumed refractive power of air of a given density. For

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[*] This communication is inserted at full length in vol. xxiv. of the *Memoirs* of the Society.

this purpose he increases the logarithms of the refractions given
Bessel's table, by the constant ·00507, in order to make the refraction at Z.D. 45° agree with his own value.   According to Bessel
himself the logarithm to be added to the quantity in table 1,
order to reduce the state of barometer and thermometer suppose
by Ivory, would be ·00412 or ·00095 less than before; but th
only indicates that Bessel supposes the refractive power of ai
under given circumstances, to be slightly less than Ivory takes i
The latter assumes this refractive power to be the same as that o
which the French tables are based, and consequently the quantitie
in these tables are rendered comparable with his own by merel
adding to these logs. the constant ·00115, which is the difference
of the log. of 29·921 inch. and 30 inch. in the heights of the
barometer supposed in the two cases respectively.'

"Ivory regarded, and I think truly, that Bessel's refractions
were to be considered as resulting from, and agreeing with, observation, and therefore as affording the best test which he could
obtain of the accuracy of his own views touching the constitution
of the atmosphere.   That they are employed in the reduction of
the *Greenwich Observations* shows the value which is attached
to them by the Astronomer Royal.

"The value of the refraction at 45° apparent zenith distance
of Bessel, as given by Ivory, is 58‴·36, but the true value is
58″·23 ; this difference is so small that without recalculating
my expressions, which would involve a very serious amount of
labour, the proper figures may be obtained, owing to the manner
in which the constant depending on the refractive power of air
enters into this expression by subtracting from the refraction, as
given in my former table, a small quantity proportioned to it.   In
this manner the first column of the following table has been
formed.   The second column has been calculated by me from
the tables contained in the *Greenwich Observations* for 1853,
recently issued, the argument being the apparent zenith distance
for barometer 30 inch. and 50° Fahrenheit.   The third column
was calculated by me from the tables formed upon Laplace's
theory, and given in the *Conn. des Temps* for 1851, by M. Caillet.   At low altitudes these tables give greater refractions than
Bessel's, but the table of errors which Bessel gives in the *Fundamenta*, p. 53, seems to indicate that his refractions are too
great near the horizon.   The fourth column gives the refractions,
which would obtain, calculated from Laplace's expressions, if the
refraction at 45° was made to agree with Bessel's, that is, they are
the numbers in col. 3 multiplied by $\frac{58\cdot23}{58\cdot39}$.   But it must be recollected that astronomers who reduce their observations by means
of M. Caillet's tables use those given in col. 3.

"My expression for the refraction, barometer 30 inch. and Fahrenheit 50°, is as follows, the coefficients of my former expression
being multiplied by $\frac{58\cdot23}{58\cdot36}$:

Ref. $= \sin \prime \{ 1130\prime\prime\cdot3\, e + 636\prime\prime\cdot8\, e^3 + 219\prime\prime\cdot9\, e^5 + 60\prime\prime\cdot4\, e^7 + 17\prime\prime\cdot8\, e^9 + 5\prime\prime\cdot5\, e^{11} + \&c. \}$

" When θ is the apparent zenith distance, $e = \tan \frac{\varphi}{2}$

$$\tan \varphi = \frac{[9 \cdot 0139814]}{\cos \theta}$$

" This expression gives for the horizontal refraction 2070″·7. Ivory says that there is great probability that the horizontal refraction is very near 2070″, and does not exceed that quantity.

" Groombridge, who made many observations for the purpose of determining the amount of the refraction near the horizon, makes the horizontal refraction for barometer 30 inch., and thermometer Fahrenheit 50°, 2075″·4, which is exactly the horizontal refraction furnished in my expression in p. 134.

" It may be concluded that the refractions which belong to the atmosphere, constituted as I have supposed, in conformity with my theory of the heat of steam and other vapours, are consistent with observation.

Mean Refractions for the Temperature 50 Fahrenheit and Barometric Pressure 30 Inches.

App. Zen. Dist.	Lubbock. 1855.	Bessel.	Conn. des Temps.	Conn. des Temps, alt⁴.	App. Zen. Dist.
10°	10″·28	10″·28	10″·31	10″·28	10°
20	21·21	21·21	21·27	21·21	20
30	33·65	34·65	33·74	33·65	30
40	48·88	48·88	49·01	48·88	40
45	58·23	58·23	58·39	58·23	45
50	69·36	69·36	69·56	69·37	50
55	83·06	83·06 ·	83·29	83·06	55
60	100·62	100·62	100·77	100·49	60
65	124·35	124·34	124·70	124·36	65
70	158·81	158·76	159·23	158·79	70
75	214·20	214·10	214·78	214·19	75
80	319·37	319·16	320·23	319·35	80
85	591·81	591·99	591·70	590·07	85
86	704·46	705·40			86
87	862·27	862·49			87
88	1094·76	1098·70			88

" As Table V. is not carried beyond 85° in the *Conn. des Temps,* I calculated from Bessel's tables the refractions for 86°, 87°, and 88°, for Fahrenheit 50°, and barometer 29·921, in order to compare them with those given by M. Caillet for those conditions in Table VI., *Conn. des Temps,* 1851, p. 58.

	Bessel.	Conn. des Temps.	Conn. des Temps, alt⁴.
86	703·63	708·80	706·85
87	860·26	868·73	866·35
88	1095·80	1103·09	1100·07

Dr. Drew exhibited a series of astronomical diagrams, representing various celestial objects, such as the phases of the planets, the craters in the moon, clusters of stars, nebulæ, &c., executed from the drawings of Mr. De La Rue, Lord Rosse, Sir John Herschel, and other original sources, and adapted to the lecture-room. The nebulæ and double-stars were depicted on a blue ground, which produced a pleasing effect.

Note on Comet II. 1855. By Dr. Donati.

In this Note Dr. Donati first gives parabolic elements of Comet II. 1855, and then assigns elliptic elements which he deduced from four positions of the comet by the aid of a method recently devised by Mossotti. The following are the positions which served as the basis of calculation; they are reduced to the mean equinox of Jan. 1, 1855, and corrected for aberration and parallax:—

	Florence M.T.	R.A.	Decl.
		$^\circ$ $\prime$ $\prime\prime$	$^\circ$ $\prime$ $\prime\prime$
1855, Jan.	3·41956	100 17 2·8	+36 19 42·0
	5·44491	107 49 57·1	36 15 48·9
	11·42931	121 9 45·1	34 39 36·1
	17·41726	127 23 38·8	+33 2 16·1

From these positions the following elliptical elements were deduced:—

Passage of the Perihelion, 1855, May 30·232563 Florence M.T.

Longitude of the Ascending Node...... 260 15 7·3 ⎱ Mean Equinox,
Longitude of the Perihelion .... 282 54 12·7 ⎰   1855·0.
Inclination .... 156 52 51·6
Perihelion Distance........................ 0·5678239
Excentricity......... .... 0·9909006

whence

Semi-major Axis .... 62·40234
Time of Revolution...... 492·95 years.

" The inclination greater than 90° indicates that the helio-centric motion of the comet is *retrograde.* Employing the usual mode of distinction we should have

<div style="text-align:center">

Longitude of the Perihelion ...... 237 36′ 1·9′

Inclination .... 23 7 8·4

</div>

" The following is a comparison between the positions calcu-lated from the foregoing elliptic elements and the fundamental positions employed in computing the orbit:—

	Obs.—Theory.	
	R.A.	Decl.
June 3	−5·2	−5·9
5	+9·8	+6·4
11	+1·4	−2·1
17	+1·8	+0·9

" After having obtained these results, I searched in the Cata-logue of Comets contained in Delambre's Astronomy with the view of ascertaining whether 493 years back (conformably to the ellipse which I deduced) a comet had pursued a path similar to that of Comet II. of the present year; and I was surprised to find that precisely in the year 1362 a comet had appeared, the motion of which was *retrograde*, and for which Burckhardt had calcu-lated the following two orbits :—

<div style="text-align:center">

Orbit I.

Passage of the Perihelion, 1362, March 11·208 Paris M.T.

</div>

Longitude of the Ascending Node ......   249°

Longitude of the Perihelion ....:........   219

Inclination   21

Perihelion Distance .... 0·4558

<div style="text-align:center">

Orbit II.

Passage of the Perihelion, 1362, March 2·333 Paris M.T.

</div>

Longitude of the Ascending Node ......   237°

Longitude of the Perihelion   227

Inclination   32

Perihelion Distance .... 0·4700

" These two orbits may be considered as the limits of the true orbit described by the Comet of 1362, of which merely gross observations were made.

" If we institute a comparison between the elements of Comet II. of this year and those of the Comet of 1362, we shall find that the two orbits sufficiently resemble each other.

" It must be borne in mind, however, that the orbit of the Comet of 1362 is uncertain, and that, if I have obtained for the second Comet of 1855 a period of 493 years, this result cannot be

considered as an absolute determination, since the small arc through which it has been possible to observe the comet may easily be made to coincide with ellipses of widely different excentricities.

" However, the coincidence of the period and the resemblance of the two orbits appear to me to render it, if not absolutely certain, at least extremely probable, that the second Comet of 1855 is identical with that of 1362."

Elements of Fides. By M. George Rümker.

(*Communicated by Professor Challis.*)

M	......	322 17 40·3	1855, Nov. 0·0 G.M.T.
π	......	63 26· 6·9	Mean equinox, Jan. 0·0
☊	......	8 8 56·2	1856.
i	......	3 11 43·6	
φ	......	8 22 25·8	
Log a ...		0·415680	
Log μ ...		2·966487	

These elements were computed from observations at Bilk, Oct. 6; Berlin Oct. 23; and Hamburg, Nov. 2 and 13.

On certain Appearances connected with the Zodiacal Light.
By Baron Humboldt.*

" In Gould's valuable American *Astronomical Journal* (No. lxxxiv., May 26, 1855), there appears a letter from the Rev. Mr. Jones, chaplain of the frigate Mississippi, containing, as the result of his observations of the Zodiacal Light in the seas of China and Japan, the conjecture of a second radiating ring of light having a relation to the moon. This conjecture is founded upon the *extra-ordinary spectacle of the Zodiacal Light simultaneously observed at both east and west horizons from eleven to one o'clock*, during several days in succession. As I observed something analogous fifty-two years ago in the Southern Ocean during the voyage of forty days from Callao, in Peru, to the port of Acapulco in Mexico, and have given only a very brief account of it in the astronomical part of my *Cosmos*, it may not be uninteresting to the members of the Academy if I laid before them an extract from my French Journal, written at sea, relating to this phenomenon, which hitherto has not formed the subject of any detailed remarks. The Zodiacal Light, and the difficult question whether we ought to attribute to a physical cause existing beyond our atmosphere the remarkable variations of light which it undergoes, while in tropical nights the smallest stars exhibit the same brightness to the naked eye, were

* Monatsbericht der Kön. Preuss. Akademie der Wissenschaften, Juli, 1855.

subjects which engaged my attention during a period of five years upon great heights among the Cordilleras, in the extensive plains or Llanos, at sea, and on both sides of the equator, as will be seen by reference to my partly published correspondence with Olbers (*Cosmos*, vol. i. p. 412.) From my ship-journal' I extract the following observations, extending from the 14th to the 19th of March, 1803, between north latitude 12° 9' and 15° 20', and chronometrical longitude 104° 27' and 105° 46' west of Paris.

" On the 17th and 18th of March the Zodiacal Light, the base of which appeared to rest upon the sun, shone with a brightness which I had never seen on any former occasion of the approach of the vernal equinox. The luminous pyramid terminated between *Aldebaran* and the *Pleiades* at an apparent altitude of 39° 5' measured above the sea-horizon, which was still sufficiently visible. The vertex was somewhat inclined towards the north; and the direction of the brightest part appeared by the compass to be west-north-west. What has struck me most during this voyage, is the great regularity with which, during five or six nights in succession, the brightness of the Zodiacal Light progressively increased and diminished. Its existence was hardly discernible during the first three-quarters of an hour after sunset, although the darkness was sufficiently great to render visible the stars of the fourth and fifth magnitude; but after 7ʰ 15ᵐ the luminous spindle appeared at once in all its beauty. Its colour was not white, like that of the milky way, but a reddish yellow, as Dominique Cassini assures us he had seen it in Europe. Very small clouds, situated accidentally towards the horizon, reflected upon the reddish ground a lively blue light. One would almost suppose he saw a second sunset in the west. About ten o'clock the light entirely disappeared; at midnight I perceived only a feeble trace of it, although the celestial vault still exhibited the same degree of transparency. *While the light was very bright in the west, we constantly perceived in the east (and this is beyond doubt a very striking phenomenon) a whitish light, which was also of a pyramidal form. The latter augmented the brightness of the sky in a very striking manner. Even the sailors were delighted with this double light in the west and the east; and I am inclined to think that this white light in the east was the reflexion of the real Zodiacal Light at setting. Both also disappeared at the same time.* Analogous reflexions frequently present themselves in our climates at sunset, but I should never have imagined that the brightness of the zodiacal light could be sufficiently strong to repeat itself by the simple reflexion of the rays. All these luminous appearances were almost the same from the 14th to the 19th of March. We did not see the Zodiacal Light on the 20th and 21st of March, *although the nights were beautiful in the highest degree.*"

These are the words of my ship-journal, containing my observations, and also the thoughts which they suggested at the time to my mind. It was in reference to what I had written down in an

unpublished ship-journal on the occasion of a voyage in the
Southern Ocean, about the beginning of the present century, that
five years previous to the publication of the interesting observa-
tions of the Rev. Mr. George Jones, I made the following state-
ment in the astronomical part of *Cosmos* :—

"On the whole the variations of the Zodiacal Light appear to
me to depend upon variations inherent in the phenomenon, upon
the greater or less intensity of the luminous processes going on in
the ring. This is proved by my observations in the Southern
Ocean, which indicated an opposite light in the heavens similar
that seen at sunset." (*Cosmos*, vol. iii. p. 589.)

I may remark further that I have been surprised at the in-
creased brightness of the Zodiacal Light upon ascending to great
altitudes. This was observable upon the lofty peaks of the
Cordilleras 10,000 or 12,000 feet high; also in Mexico, in Jan-
uary 1804, at altitudes of only 7000 feet; and from the Cloister
of Mount Cenis, where I remained with Gay Lussac several
nights (in March 1805) at an altitude of 6350 feet, for the purpose
of determining the intensity of the magnetic force during very
intense cold, and ascertaining the quantity of oxygen contained in
the atmosphere. It was, consequently, seen both in tropical and in
temperate latitudes. But the variations in the brightness of the
phenomenon cannot, according to my experience, be accounted for
solely by the constitution of our atmosphere. There remains much
still to be observed relative to this subject.

The following is a copy of the letter to which Baron Humboldt
refers :—

(*From the Rev. George Jones, U.S.N. to the Editor of the Astronomical
Journal, Cambridge, U.S.*)

"In my recent cruise in the U.S. steam-frigate Mississippi,
chiefly in the China and Japan seas, but taking us also around the
globe, I had excellent opportunities for observation of the Zodiacal
Light. This light, you know, appears to the best advantage within
the tropics, where it stretches upward to a great elevation, and is
a remarkable object; but it is also very desirable to observe it in
high latitudes; and in this also I was favoured, as our voyaging
extended from 41° north to 53° south latitude; and in some in-
stances our transitions, for weeks together, were very rapid, thus
giving me opportunities for observing whether any parallax could
be made or not.

"I was also fortunate enough to be twice near the latitude of
23° 28' north, when the sun was at the opposite solstice, in which
position the observer has the ecliptic, at midnight, at right angles
with his horizon, and bearing east and west. Whether the latter
circumstance affected the result or not I cannot say; but I then
had the extraordinary spectacle of the Zodiacal Light, simul-
taneously at both east and west horizons, from 11 to 1 o'clock, for
several nights in succession.

"In the first part of our cruise my observations were of a

desultory character; but I soon began to see the necessity of great precision, and accordingly constructed star-charts from a celestial globe (a small but excellent one) that happened to be on board, which charts I afterwards had cut in wood at Canton; and thus I was furnished with materials for accurately recording all the changes of this phenomenon, not only in successive nights, but also in the successive hours of the same night. My rule was, to draw on my charts the boundaries of the Zodiacal Light as exhibited among the stars, with such annotations as the case required; then, again, do the same after an interval of an hour or half-hour; and so to continue, generally as long as the boundaries could be made out reliably; then, if the morning admitted it, to resume observations at the earliest possible hour, and so to proceed until the dawn. I have thus, in many instances, observations for every hour of the night.

"At an early period I began to query whether the moon, when near its full, might not give a Zodiacal Light: and at last, when I had gained more experience in observing, and in the peculiar character of this light, I was able to get, at different periods, fourteen reliable observations of what I think must be considered a Zodiacal Light produced by the moon. I have also two records of a distinct Zodiacal Light produced by the joint action of the sun and moon, *i. e.* at the hour when the moon, then near its first quartering, was about 65° above the western horizon; the reflexion from the combined light of the sun and moon being sufficient to overpower the moonlight proper, and thus to produce a decided stream of light in the sky within the Zodiacal-Light boundaries. The latter of these observations was the more remarkable, inasmuch as the moon was then *without* the boundary of this joint reflected light.

"You will excuse my prolixity in stating these varieties of observation, for the conclusion from all the data in my possession is a startling one. It seems to me that these data can be explained only by the supposition of a nebulous ring with the earth for its centre, and lying within the orbit of the moon. This conclusion seems to evolve itself, — 1st, from the simultaneous midnight east and west observations, which preclude the possibility of a ring around the sun *within* the earth's orbit; 2dly, from the great hourly lateral changes (often semi-hourly) in the boundaries of the Light, caused by the observer's change of place, in that time, as regards the ecliptic or axis of the Zodiacal Light, which lateral change in the Light is too great to allow of our considering it at a distance of 170,000,000 of miles, as its lower end would be, near dawn, if it is a ring around the sun and beyond the earth's orbit; and 3dly, from the moon's Zodiacal Light, if real, which I think it is. That it is a ring, the unbroken continuity of my observations satisfactorily determines. For more than two years, I never failed to see this Light, evening and morning, when the moon and clouds did not interfere ; and, except one evening, I have continuous records of this kind.

"I could get no parallax; but. on the contrary, as we went south, the boundaries of the Zodiacal Light changed with us to the south among the stars; and so, *vice versâ*, towards the north, caused, doubtless, by the ring's presenting new portions of its wide reflecting surface to the sun's light.

"GEORGE JONES,
"Chaplain U.S. Navy."

Brooklyn, May 17, 1855.

Description of New or Improved Instruments for Navigation and Astronomy. Exhibited at the Paris Universal Exposition of 1855, by C. Piazzi Smyth, Astronomer Royal for Scotland, Professor of Practical Astronomy in the University of Edinburgh. Edinburgh: June, 1855.

The instruments described in this *brochure* are arranged in four distinct classes: viz. 1. Instruments of navigation; 2. Instruments of nautical astronomy ; 3. Instruments of terrestrial astronomy ; 4. Instruments of cosmical astronomy. The total number of instruments described is twenty. We extract the following description of an instrument which is classed under the head of Cosmical Astronomy :—

"No. 18. *Edinburgh Equatoreal, Universal.*—In all the equatoreally mounted telescopes intended for transportation to and employment in various latitudes, *i.e.* in all with which I have yet had the fortune to meet, there has invariably. been this feature of awkwardness ; viz. that the centre of gravity of the instrument was not coincident with the centre of the arc for latitude adjustment, and was, indeed, considerably removed from it.

"Hence it arose that when one of these portable equatoreals, constructed to suit the latitude of London, was transported to the Cape of Good Hope, and had its polar axis sloped to suit the lower position there of the celestial pole, the centre of the weight of the instrument was in consequence thrown so far beyond the centre of the support, as to make it dangerously unsteady, or even incapable of standing.

"The present equatoreal is accordingly devised to meet this difficulty, by having the centre of gravity of telescope, polar axis, and, in short, the whole instrument, accurately concentric with the arc for the latitude adjustment. By this method it is brought about, that the equatoreal can be adjusted to suit either a polar or an equatoreal latitude, without any disturbance of the equilibrium of the stand.

"The little instrument exhibited may be regarded as a model of a large one, with certain proportions altered ; but it was specially constructed for actual use with the naked eye, in observations of the zodiacal light and the courses of shooting stars."

An Introduction to Practical Astronomy, with a Collection of Astronomical Tables. By Elias Loomis, LL.D., Professor of Mathematics and Natural Philosophy in the University of the City of New York. New York, 1855.

The object of this work is to provide amateurs of astronomy, individuals engaged in astronomical expeditions and Government surveys, &c. &c., with a manual containing an explanation of the methods generally employed in the ordinary class of astronomical computations. The various processes are illustrated by a copious collection of examples, and a series of tables is given to facilitate the labours of the computer. It may be remarked that the work is very lucidly drawn up and that a due regard to practical utility is visible in every page.

Results of Astronomical Observations made at the Observatory of Harvard College, under the Direction of William Crouch Bond, A.M. (Zone Catalogue of 5500 Stars, situated between the Equator and 0° 20′ North Declination, observed during the Years 1852-3), *Cambridge, U.S.,* 1855.

In an Introduction extending to ninety-seven pages the author gives a complete description of the instrumental means, and of the methods of observation and reduction, employed in these observations. The plan of observation adopted for the zones contained in this volume includes the determination of the right ascension and declination of all stars from the equator to ρ° 20′ of nórth declination to the eleventh magnitude, and as many of the twelfth as could be conveniently observed in their passage without interfering with the observation of brighter stars; the position of each has been twice determined, as a rule, both in right ascension and declination, by observations on different nights.

The present volume contains sixty-two zones, comprising between five and six thousand stars, for all of which the reductions have been applied to refer them to the mean equinox for the beginning of the year 1852 or 1853. In the recording of the right ascensions the electro-magnetic method was employed. The following description is given of this process:—

" All the observations of right ascensions have been recorded by the electro-magnetic method, which is perfectly adapted to the wants of the astronomer in a work of this nature. In three most important requisites it has unquestionably the advantage over any of the plans hitherto used,—it is more accurate in its results; it is superior in point of convenience, and in this respect recommends itself to the observer, relieving him from much labour, and contributing to the ease and comparative comfort with which the work can be prosecuted; lastly, the time necessary for completing an observation is greatly shortened, and thus an opportunity is afforded for repetition, or for determining the position of more new objects than could otherwise be included.

"The first step preparatory to the observation of a zone is to place the record-paper on the cylinder of the spring-governor, and to adjust the galvanic connexions, recording-pen, &c. The equatoreal is then set upon a star, previously selected for the starting point of the zone, of which the right ascension and declination are known; for convenience some bright star is usually chosen. The focal adjustment is now examined, and made to satisfy the condition that the parallax of the image of the star, with reference to the divisions of the scale, should be as nearly as possible eliminated.

"The zero of position of the right-ascension lines is next determined by several passages of a suitable star, and the position circle of the micrometer firmly clamped at the required reading. In this situation the eye-piece has a sliding motion across the scale in the direction of right ascension,—a very useful contrivance, which enables the observer to command the field preceding that which is the immediate scene of observation, and gives time for estimating magnitudes, &c., before the star enters the scale.

"The connexion of the battery with the clock and recording apparatus having been made, and the indications of the barometer and external thermometer read off, the telescope is clamped in right ascension and in declination, so that the zero star shall cross the scale at the reading corresponding to the minutes and seconds of its mean declination for the beginning of the year. As each star approaches the scale, its magnitude is noted, and any peculiarity of appearance, colour, &c., if such are presented. When crossing the scale its declination is read off, and immediately after a signal is given with the break-circuit key in the hands of the observer, which is recorded upon the sheet on the cylinder of the spring-governor. This signal announces the approach of the star to the right-ascension wires; the two passages are noted in succession, occurring at the equator at an interval of four seconds of time. The signal for the instant of their occurrence is given by a tap of the finger on the break-circuit key.

"For every fifth or sixth star the signals are varied by 'breaks' and 'dots,' for which a parallel record is kept in the columns containing the readings of the declination scale; this is done to ensure the identification of the right ascensions and declinations, and to render certain their correct application, each to its proper star.

"Opportunities are taken to note down striking peculiarities in the distribution and number of stars, the nebulæ, clusters, and double stars. In this manner the observer, with the aid of an assistant, whose office is to record the declinations, magnitudes, &c., has it in his power to give the elements of position and magnitude of each star, at the rate of six or seven stars to a minute; the average frequency of observation is about two to a minute. It has been the usual practice not to extend the zone

* For a description of Mr. Bond's recording apparatus see Monthly Notices, vol. xi. p. 163.

much beyond the limits of two hours in right ascension. On the following night the observation of the zone is repeated. In this instance the assistant states the magnitude and declination by the previous night's work; the observer has then an opportunity for correcting his last night's results by comparing them with the stars themselves, as they pass the field. The record sheets are now to be read off, and their indications transferred to the note-book in a column opposite to that of the declinations. The counting of the hours, minutes, and seconds on the sheet, is commenced at a point which gives the right ascension of each star differing by a small amount—usually by less than two seconds—from its place referred to the mean equinox for the beginning of the year. The sheet measures twelve inches by twenty. One side of such a sheet is occupied with observations extending over two hours and twenty minutes. The hours and minutes are entered from the side of the sheet, and the seconds from the top."

The *Nautical Almanac* for 1859 has recently been published. Besides the usual quantity of matter, it contains the ephemerides of the small planets for 1856, in the form of a Supplement, as mentioned in the last number of the *Monthly Notices*.

At the meeting of the French Institute, held on the 23d of July, Sir John Herschel was elected one of the eight Foreign Associates of the Institute, in the room of the late M. Gauss.

The Royal Society has awarded the Copley Medal this year to M. Leon Foucault, for his various researches in experimental physics. Of the two Royal Medals one has been awarded to Mr. Hind, for his discovery of ten planetoids, the computation of their orbits, and various other astronomical discoveries; and the other to Mr. J. O. Westwood, President of the Entomological Society, for his various monographs and papers on entomology.

Discovery of a new Comet. By M. Bruhns.

A new comet was discovered by M. Bruhns on the 12th of November, at 15¼ʰ. Shortly afterwards he obtained the following observation of it:—

	Berlin M.T.	App. Comet's R.A.	App. Comet's Decl.
	h m s	o ' "	o ' "
Nov. 12	17 21 53·3	149 1 25·7	+2 7 15·3

The comet resembles a faint nebula; it has a daily motion in right ascension of about −20′ in arc; and is almost motionless in declination.[*]

   * See the elements of this comet on next page.

Elements of the New Comet discovered by M. Bruhns.

By M. George Rümker.

(*Communicated by Professor Challis.*)

T ...... Nov. 25·66041, 1855, G.M.T.

$\begin{array}{l} \text{τ} ...... \quad 85 \ 21 \ 41 \\ \text{☊} ...... \quad 52 \ \ 2 \ 47 \end{array} \Big\}$ App. equinox, Nov. 15.

i 10 16 29

Log *q* . 0·088070

These elements were computed from observations at Berlin, Nov. 12; Bi...
Nov. 15; and Hamburg, Nov. 20.

ERRATA, VOL. XV.

Page 193, line 13, *for* memoria, *read* memorie.

— 195, — 10, *for* resolveri, *read* resolvere.

— 196, — 15, *for* accidente, *read* accidenti.

— 196, — 22, *for* risposta dei, *read* risposta al.

CONTENTS.

M. J. Johnson, Esq. President, in the Chair.

J. G. Barclay, Esq., 54 Lombard Street, and
William Monk, Esq., St. John's College, Cambridge,
were balloted for and duly elected Fellows of the Society.

Vol. XV. of the *Monthly Notices* has been recently published.
Its price is fixed by the Council at 2s. 6d. to Fellows of the
Society, and 5s. to the public. It need hardly be mentioned that
this Journal offers a ready channel of publication to observers of
all classes, and to every individual who devotes his attention more
or less to astronomical pursuits.

Note on the Occultation of Stars by Saturn.
By the Astronomer Royal.

" In No. 920 of the *Astronomische Nachrichten* (1854, October
10) M. Winnecke pointed out the probability of the occultation
of two stars, in the years 1854 and 1856 respectively, by *Saturn*.

" The first was the probable occultation of Lalande 9362, Bessel's
Zone 343, on 1854, November 13, visible in Europe. Unfavourable
weather prevented the observation of this phenomenon at Green-
wich; I know not whether it was looked for in any other obser-
vatory.

" The second probable occultation is that of Lalande 13545,
Bessel's Zone 279, on 1856, September 9, at about $8^h 20^m$ Berlin
mean time. This occultation will not be visible in Europe. The
positions of the Sun and *Saturn* at that time are nearly as fol-
low : —

	h m		° ′
Sun's R.A	11 13	N.P.D.	84 57
Saturn's R.A.	6 53	N.P.D.	67 46

" The Sun is then west of the meridian of Greenwich by $7^h 29^m$,
and *Saturn* is west of the same meridian by $11^h 49^m$.

" A great circle, drawn through the centre of the Sea of Okhotsk
and passing a short distance east of the eastern coasts of Australia,
defines the line on which the sun is rising. A great circle, passing
a little to the west of Calcutta and a little to the west of the west-

ern coasts of Australia, defines the line on which *Saturn* is rising, 1g.
Between these two great circles the phenomenon may be seen.

" The limits which I have indicated do not include Bombay   or
Madras.   But they include Calcutta and every British settlement
east of Calcutta; especially Singapore, the coasts of China, and
the whole of Australia.

" Perhaps the circulation of this notice, in the monthly publica-
tion of the Society, may induce some persons to watch for a phe-
nomenon which at any time would be highly worthy of attention,
but which, in reference to discussions now pending, might be one
of very great interest.

" 1855, *Dec.* 10."

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*Observations of the Zodiacal Light at Highfield House Observa-
    tory, near Nottingham.*   By E. J. Lowe, F.R.A.S.

" The following are observations which I had intended for for-
warding to the Society some months ago; they form the continu-
ation of several reports upon this subject, which I have before had
the honour to communicate.   The chief interesting feature is an
inner line of *brighter light* seen on the 25th of February, 1854.
It has struck me that this line may be the edge of a ring, and in
this case the zodiacal light will be *a ring of light* surrounding the
sun, so inclined as to be in the same (or nearly) plane with the
zodiac.   I am aware that several eminent astronomers have seen
weighty reasons why this should not be the case, and I therefore
refrain from entering further into the subject at the present mo-
ment, and shall content myself with giving merely the observations
themselves.

<center>Epoch 1854, February 26ᵈ 7ʰ 30ᵐ G.M.T.</center>

became very brilliant by impulses.   ♉ 28°, altitude 45°, conside-
rably brighter than the galaxy.   Last night there was a brisk
breeze, to-night calm.   The phenomenon was faint at 7 o'clock;
was still brilliant at 8 o'clock, the pulsations being strongly
marked, and less bright at 8ʰ 15ᵐ.

<center>Epoch 1854, February 18ᵈ 7ʰ 30ᵐ</center>

visible for a few minutes, the axis passed midway between *α Arietis*
and the *Pleiades*.

<center>Epoch 1854, February 25ᵈ 7ʰ 30ᵐ</center>

scarcely perceptible.

<center>Epoch 1854, February 25ᵈ 7ʰ 33ᵐ</center>

very brilliant.

<center>Epoch 1854, February 25ᵈ 7ʰ 35ᵐ</center>

faint.

Epoch 1854, February 25^d 7^h 45^m

time 'of greatest brilliancy. The pulsations of brightness more marked than I had ever before observed them, and at the times of maximum brightness, the phenomenon was altogether more brilliant than I had before seen it. The cone of light w&s *straight* along the northern edge, but decidedly *curved* on the southern edge.

Epoch 1854, February 25^d 8^h 0^m

The north edge cut the horizon 10° N. of W., and the south edge about 18° S. of W. If the N. edge were produced it would pass through γ *Piscium*. The axis cut the horizon 5° S. of W., the apex being at an altitude of 44°. The axis produced would pass 1° N. of the *Pleiades*.

Epoch 1854, February 25^d 7^h 45^m (continued).

At this time the position of the edges amongst the stars was noted. The cone passed S. of γ *Arietis* (but nearly touched that star at 7^h 40^m, and again at 8^h 0^m, yet only for a second or two). It mostly cut on the N. edge η *Piscium*, and on the S. edge ο *Piscium;* at times both these stars were *within* the cone of light distances varying from 15′ to 30′. The stars μ, ζ, ι, and δ *Piscium* (within the cone) were scarcely visible, and occasionally invisible. There was a line of light, more brilliant than any other portion of the phenomenon, which extended along the whole length of the cone to within 5° of the apex, being brightest at about the altitude of ι *Piscium.** The cone passed generally within 3° of γ *Pegasi,* yet occasionally within 1° of that star, whilst at times it was 4° distant from it. The zodiacal light was twice as brilliant as the brightest portions of the milky way; the latter, however, was not brilliant, although the night was very starlight. At 8^h 16^m a falling star, composed of a number of fragments, moved from γ *Pegasi* towards ι *Piscium*, and went out *exactly* as it touched the N. edge of the zodiacal light, leaving the impression that the phenomenon hid it from view; the meteor was only equal to a star of the fourth magnitude; stars of this magnitude were not visible *within* the cone of light.

Epoch 1854, March 1^d 7^h 30^m

faint, broader than on the 26th, yet more diffused, and the edges ill defined.

Epoch 1855, January 17^d 6^h 50^m

brilliant, and well defined near *Aquarius*. The N. edge cut ζ *Aquarii* and the S. edge τ *Aquarii;* the con ιextended to the altitude of α *Andromedæ*, in the direction of the *Pleiades*.

Epoch 1855, February 12^d 7^h 0^m

magnificent; well marked on the edges; rose to about the altitude of α *Arietis*, pointing towards the *Pleiades*. The N. edge was 1° N. of β *Pegasi*.

* The author forwarded a sketch of the cone exhibiting the position of this line.—EDITOR.

Epoch 1855, February 14^d 7^h 0^m .

faint and confused.

Epoch 1855, February 17^d 7^h 0^m

olerably bright, yet confused, owing to a strong auroral glare.

Epoch 1855, March 7^d 7^h 50^m

very brilliant, tolerably steady in its light; N. edge sharp and
well defined, passing immediately over γ *Arietis*, and covering
η *Piscium*. S. edge confused. The apex extended to ζ *Arietis*.

Epoch 1855, March 18^d 7^h 30^m

bright but confused.

Description of an Out-of-doors Equatoreal Stand for Telescopes.
By the Rev. Samuel King, M.A.

" The accompanying photograph will give a general idea of the
construction of this instrument, which is a modification of the
parallactic ladder described in Admiral Smyth's invaluable
Cycle. The chief novelty is the introduction of circles of right
ascension and declination divided on slate.

" Upon a pier of brick-work, built in cement upon a substantial
foundation, is placed a stone cube, having its northern edge cham-
fered off at an angle equal to the co-latitude. On this sloping face
is securely fixed the brass socket or collar (with antagonist screws
for adjustment) which carries the south pivot of the polar axis.
The collar for the north pivot is sunk in a square block of ma-
hogany, having strong brass cylindrical trunnions horizontally
situated on opposite sides, the axes of which are in a line with the
collar. These trunnions work in sockets formed for them in the
top of each leg of the supporting shears;—the collar is thus
brought to bear true upon the shoulder of the pivot in any position
of the legs while roughly adjusting for altitude.

" The polar axis is a strong, double mortised, oblong frame,
within which, and acted upon by a small double tackle, slides
another frame, like a sash-window, carrying the cradle or trough,
with Y's at either end, in which the telescope is securely fixed, so
that the lines of the polar and declination axes intersect in its
centre of gravity, and it balances accurately in every position.

" On the foot of the polar axis is fixed the slate hour-circle, 15
inches in diameter, divided on the limb with considerable accuracy
(by means of a lathe with division-plate and index) to four minutes
of time, allowing a very fair estimate to half a minute (without a
vernier, which I intend some time adding). The declination
circle, also of slate, is 12 inches diameter, divided to 30′.

" This apparatus has been exposed to all the vicissitudes of the
weather for more than a year and a half, without requiring any

material alteration in the adjustments. It carries an excellent telescope of 5 feet focal length and 4¼ inches aperture, by Mills of Pentonville; and with a power of 72 I can find, without fail, anything I look for by day or night. By means of a pulley-wheel beneath the hour-circle, connected by a catgut band with a smaller pulley acted upon by a tangent screw, I get an excellent slow motion in R.A., to which I hope shortly to attach a jack to act as a clock.

"The wood-work is of clean-grained Honduras mahogany. The cost of the whole to me was about 4*l.* The brass work and division of the circles, which I executed myself, I set down at 2*l.* additional. This is altogether a very small cost for the facilities which the apparatus affords to the astronomer whose means are limited; and there are few places now in which a suitable lathe, and a sufficiently skilful workman, cannot be found for the execution of all that is needed.

"I have been as concise as possible in the description of this affair; but it will give me much pleasure to furnish more particular details to any gentleman who may be desirous of them. It may be added that the framing of the shears being secured by brass thumb-screws, the whole machine may be taken to pieces and removed in five minutes, with the exception of the pier, which, with a little contrivance, might be made to answer the additional purpose of supporting a sun-dial.

"*St. Aubin's, Jersey, December* 18, 1855."

Occultations of Stars by the Moon observed at Highbury.
By T. W. Burr, Esq.

Latitude 51° 33′ 45″·1 N. Longitude 23″·8 W.

1855.			Sidereal Time. h m s	
March 5th γ *Virginis*	Preceding Star	Immersion	9 13 46·4	Bright Limb
,,	Following Star	,,	9 14 6·4	
,,	,,	Emersion	9 29 47·4	Dark Limb
April 23d λ *Cancri*		Immersion	12 57 59·8	Dark Limb
Oct. 24th ε *Piscium* . . .		,,	23 56 54·6	Dark Limb. Moon very nearly full

"The two latter occultations are good observations as to time. In the case of γ *Virginis* the clock-error was not so accurately known; but the observations are recorded for the sake of drawing attention to the length of time during which the second star remained attached to the edge of the moon's disc. The preceding star disappeared instantaneously; but the following one, which arrived at the moon's edge in 4ˢ or 5ˢ, remained attached to it until 20ˢ had elapsed from the immersion of the first star, when the

second also disappeared. There was no projection of the star or
the moon's surface; but it appeared to assume a planetary disc,
part of which was hidden, and the rest remained as a slight ex-
crescence on the moon's edge. Probably, had the immersion taken
place at the dark limb, the whole of the spurious disc would have
been seen projected on the surface, as the impression produced on
my mind was that such was now the case, only the bright surfaces
of the moon and star could not be distinguished from one another.

" In consequence of the moon's position being low in the east,
the line joining the stars formed nearly a right angle with a
tangent to the moon's disc; so that the longest possible time
elapsed between the immersions: and the fact that the stars
passed behind a very small portion of the moon's body, so that
they might at first do little more than graze the edge, throws
some light on the unusual period of time during which the attach-
ment continued.

" The telescope was my equatoreal by Ross, 4 feet focal
length, 3⅝ inches aperture. Power, 173."

On the Dimensions of the Rings of Saturn.
By the Rev. R. Main, M.A.

The author commences by stating, that in the winter of 1852-3
he executed a series of measures of the dimensions of the rings of
Saturn, with a double-image micrometer, similar in principle,
but not identically the same, as that used in the measures which
he executed in 1849 for determining the form of the planet, an
account of which is published in vol. xviii. of the *Memoirs* of the
Society. In the beginning of 1854 he repeated his measures of
the dimensions of the rings. Both sets of measures will be found
printed and reduced in the volumes of the *Greenwich Observations*
for 1852 and 1854, which also contain an account of the con-
struction of the micrometer, the method of using it, the degree of
accuracy and delicacy of measurement attainable by it, and other
circumstances which a critic would desire to become acquainted
with.

The author, in giving a synopsis of the results of his measures
as inserted in the *Greenwich Observations*, remarks, that he
attaches no great weight to the measures of the breadth of the
black division, on account of the extreme difficulty and uncertainty
of the contacts of the point of reference with its borders. In
fact, the aperture of the object-glass being only 6¼ inches, and
only half of the light of the planet being employed for the forma-
tion of each image, all the measures were very difficult from the
uncertainty of determining the exact contact of the assumed point
of reference, namely, the extreme edge of the exterior bright
ring, with the borders of the inner ring, and of the ball whose

dimensions were required. This difficulty was much increased by the confusion arising from the superposition of the different portions of one of the images upon the other, which obliged the author to deviate from the usual method of making contacts of images on opposite sides on account of its impracticability. He has consequently given in his synopsis of results only the position of the centre of the black division.

The author's measures for 1852–3 are included between December 30, 1852, and February 19, 1853. The following is the mean for January 11, 1853:—

<div align="center">

Distance of assumed centre from

East exterior edge of outer ring	21·36
— centre of black division	19·19
— interior edge of inner bright ring	14·52
— edge of ball	8·79
West edge of ball	9·06
— interior edge of inner bright ring	14·50
— centre of black division	18·62
— exterior edge of outer ring	21·64

</div>

Or, assuming with M. Otto Struve that the centre of the ball of *Saturn* is coincident with that of the rings, we have, by taking the means of the measures on the east and west sides of the planet, the following results for 1853, January 11, when the log. distance of *Saturn* from the earth was 0·94267:—

<div align="center">

Distance of centre of planet from

Edge of ball ...,...	8·98
Interior edge of inner bright ring	14·51
Centre of black division	18·91
Exterior edge of outer bright ring	21·50

</div>

Hence, subtracting the semi-diameter of the ball from the other quantities, and calling the distances of the edge of the ball from each of the other measured parts in order, according to M. Struve's notation, ad, ae',[*] and ag, we have,—

$$ad = 5''·53 \qquad ae' = 9''·93 \qquad ag = 12''·52$$

Or, reducing these results to those which would have been observed at the mean distance of the planet 9·5389 (or log. $^{-1}$ 0·97950), assumed by M. Struve, the measures are,—

$$ad = 5''·08 \qquad ae' = 9''·12 \qquad ag = 11''·50$$

---

[*] ae' is the mean of ae and af in M. Struve's figure.

The observations of 1854 extend from February 2 to March 17; the following is the mean value of the results for the epoch February 23, 1854:—

Distance of assumed centre from

East exterior edge of outer ring	20ʹ62
— centre of black division	17·44
— interior edge of inner bright ring	13·75
— ball	8·01
West edge of ball	8·66
— interior edge of inner bright ring	14·27
— centre of black division	18·29
— exterior edge of outer ring	20·59

The means of the measures given above correspond to the log distance of the planet 0·96236, and at this distance we have therefore (taking the means on each side of the centre as before),—

Distance of centre of planet from

Edge of ball =	8ʹ34 (semi-diameter of ball)
Interior edge of inner bright ring =	14·01
Centre of black division =	17·87
Exterior edge of outer bright ring =	20·61

Hence, at this distance, $ad = 5''·67$, $ae' = 9''·53$, $ag = 12''·27$. And at the mean distance of the planet, 9·5389,—

$$ad = 5''·46 \qquad ae' = 9''·17 \qquad ag = 11''·81$$

Hence the general results of the measures, compared with the measures of M. Struve, are the following:—

	Main. 1852–3.	Main. 1854.	Struve.
$ad =$	5ʹ08	5·46	3ʹ65
$ae' =$	9·12	9·17	8·52
$ag =$	11·50	11·07	11·07

Or, taking the mean of the two sets of measures, since they have on the whole pretty nearly equal weights,—

	Main. 1852–3.	Struve. 1854.	M-S.
$ad =$	5ʹ27	3ʹ65	+ 1ʹ62
$ae' =$	9·15	8·52	+ 0·63
$=$	11·28	11·07	+ 0·21

With regard to these results, the author remarks that the differences increase rapidly on approaching towards the limb of the ball from which they are reckoned, and seem to indicate that, either by M. Otto Struve or by himself, large arcs are measured on a different scale from small ones.

The author remarks that, so far as his measures can be depended on, they do not give any support .to the hypothesis of Otto Struve, concerning the gradual diminution of the interval between the ball of the planet and the inner edge of the interior bright ring; and if there were no discrepancies in the measures which made him diffident of presenting them to astronomers as capable of settling a point of so much delicacy, he should be fully satisfied that M. Struve's idea was not yet sufficiently proved. He was fully convinced, however, of the uncertainty of many of the individual measures from the confusion of the images, and of their consequent inferiority to those made for the determination of the form of the planet in 1849, and also to the measures for deter-. mining the length and breadth of the exterior edge of the outer ring. But what more especially impressed him with the necessity of receiving the results with caution, was the circumstance that the final measure for the ball of the planet is considerably smaller than that which he deduced in 1849, and which agrees with the value generally adopted by astronomers.

Taking the means of the east and west semi-diameters of the ball as measured in 1852–53, and reducing them to the mean distance assumed by M. Otto Struve, namely, 9·5389, the author obtains 8″·20 for the final value of the semi-diameter of the ball.

Treating the measures of 1854 in the same way, he finds the resulting value of the semi-diameter to be 8″·02. He remarks that the agreement between this result and that deduced from the measures of 1852–3, is sufficiently close to show that they are a fair representation of observations of the ball made by his own eye with the double-image micrometer, and in the manner previously explained when the ball was seen surrounded by his ring. The mean of the two results above mentioned (8″·20 and 8″·02), when compared with the corresponding value of the semi-diameter deduced by him in 1849, namely, 8″·75, indicates a difference of no less than 0″·64; a quantity certainly too great to be the result of accident, and undoubtedly attributable to the different circumstances under which the measures were made. M. Struve's value of the semi-diameter of the ball is 8″·80, a result agreeing almost exactly with that deduced by the author in 1849.

The author next proceeds to give his measures of the extreme length and breadth of the exterior edge of the outer ring. He remarks that these measures are much superior in accuracy to the others, the outer edges of the system of rings presenting no impediment to the ordinary use of the double-image micrometer. The mean of the measures made in 1852–3 gives 39″·67 for the diameter of the exterior ring; the mean of the measures made in

b

1854 gives 39″·76; consequently the mean of the entire collection of measures is 39″·72.

The author also deduces from the measures of the minor axis of the exterior ring the corresponding values of the major axis, by taking into account the elevation of the earth above the plane of the ring. The mean value of the diameter of the ring thus found is 39″·75, agreeing almost exactly with that determined by direct observation. This result the author considers to be definitive, and comparable with the value of the semi-diameter of the ball obtained by him in 1849.

Having thus given the general results of the measures of the rings, the author proceeds to examine the ratio which the dark space intervening between the inner bright ring and the planet bears to the breadth of the bright rings. It has been mentioned that he found the breadth of the dark space to be 5″·27, and that of the bright rings to be 6″·01 ; consequently the ratio of the two quantities is 0·87. On the other hand, M. Otto Struve found the breadth of the dark space to be 3″·65, and that of the rings 7″·42 —2, which give a ratio of only 0·49.

The difference between the values of the ratio thus found being too great to be passed over without any attempt at expla- nation, the author proceeds to substantiate his own measures by other considerations. He had already alluded to the difficulty of making accurate contacts for the interior edge of the inner ring and the ball, on account of the confusion of the images. The difficulty was very much greater when the object of reference was beyond the centre than when it was near the outer edge of the system of rings, and the measures would consequently be less accurate. From fresh trials, which he made during the present opposition of the planet (December 1855), he felt convinced that at the *entering* side of the point of reference the measures can be made with confidence, but cannot be executed so unexceptionably on the other side. He therefore discusses the two sets of measures separately.

The measures of 1852–3 give him 0·95 for the ratio of the dark space to the breadth of the rings ; the measures of 1854 indicate the same ratio to be 0·97. A curious circumstance, however, connected with both sets of measures, is this,—in several instances the breadth of the bright rings is conspicuously less than the dark interval, and the difference between them steadily decreases till the two are equal; then the breadth of ring becomes greater than the dark interval, and their ratio steadily increases. This curious law, developed equally well in both sets of observations, can scarcely be the result of accident. It probably depends on the change of optical appearance of the planet during the period of observation, and perhaps in some degree, also, on the training of the eye of the observer during the time. The author adds, however, that it would be a very bold assumption if, on the strength of these observations, which are incomparably better than the early observations on which M. Struve rests his

theory, we were to conclude that the system of *Saturn* really underwent periodical changes of the magnitude thus indicated.

The author was fortunately enabled to avail himself of some favourable evenings during the present opposition (December 1855) to make an extensive series of observations, with direct reference to the relation of the breadth of the system of bright rings to that of the dark interval.

A numerous series of measures, executed on the evening of the 5th of December, gave him 1·08 for the ratio of the dark space to the breadth of ring; a similar series of measures, executed on the 6th, gave him 1·11 for the value of the same ratio. It has been mentioned that the values of the semi-diameter of the planet which the author obtained from his recent observations, are sensibly smaller than the value of the same element which he deduced in 1849, during the disappearance of the ring. He remarks that this probably arises from the glare of the ring, which prevents the true border of the planet being observed. The circumstance at all events suggests a correction which must be applied to the observed values of the dark space between the planet and the bright rings. The author here appends a table, by which he shows that the application of this correction causes the disappearance of the curious anomaly in the ratios between the values of the dark space and the breadth of the rings. The following are the final results deduced by him from the observations of 1852–3,—

$$\frac{\text{Corrected breadth of dark space}}{\text{Breadth of rings}} = 0\text{·}838$$

and from the observations of 1854,—

$$\frac{\text{Corrected breadth of dark space}}{\text{Breadth of rings}} = 0\text{·}844$$

The agreement of these results leaves nothing to be desired. We have then the following definitive values for the mean distance of the planet from the earth previously assumed,—

Diameter of exterior edge of outer ring = 39·73
Diameter of inner edge of inner ring = 27·65
Equatoreal diameter of ball = 17·50

The author next examines the question relative to the dimensions of the black division. His final conclusion is, that the diameter of the circle coinciding with the centre of the division amounts to 36″·15.

He remarks, that there is still one circumstance connected with the observations of the planet which does not at present admit of explanation. Each of the results of the present year is quite equal in weight to either of the results deduced from the totality of the observations of 1853 and 1854; the two results agree

with each other with all desirable accuracy ; and yet the ratios indicated by them are very much greater than those of 1853 and 1854. He adds, that it would be useless to speculate at present upon the cause of this discordance ; but he looks forward with interest to the observations which he hopes to make at the present opposition in the expectation of obtaining some solution of the difficulty. He concludes with the following remarks : —

"For the present, I can only express my opinion that the results which are given exhibit with tolerable accuracy the dimensions of the *Saturnian* system, and that no change whatever has taken place in the system since the time of Huygens. If this opinion be correct, it will follow that M. Struve's measures of the breadth of the dark space between the bright rings and the ball have been influenced by some circumstance which has not affected the distance between the ball and the outer edge of the outer ring, for in this latter particular my measures are almost coincident with his. Under these circumstances there is little doubt that M. Struve will be anxious to seize the favourable opportunity that presents itself at the present opposition of the planet for repeating his measures in a way that will prevent the possibility of doubt ; and I may at the same time direct the attention of such English astronomers as are furnished with good telescopes equatoreally mounted to the interesting circumstances which I have attempted in the present memoir to elucidate."

On the Attraction of the Himalaya Mountains and of the elevated Regions beyond them, upon the Plumb-line in India. By the Venerable John Henry Pratt, M.A., Archdeacon of Calcutta.*

The author of this paper commences by remarking that the results of the measurement of the great arc of the meridian of India afford undoubted indications of the disturbing influence exercised upon the direction of the plumb-line by the attraction of the Himalaya Mountains and the elevated regions lying beyond them. Thus, the amplitude of the northern division of the arc included between Kaliana and Kalianpur, when determined by astronomical observations of latitude at the two extreme stations, was found to be $5° 23' 37''·058$; whereas, when computed geodetically, assuming the usually admitted values of the major axis and ellipticity of the earth, the value of the amplitude appeared to be $5° 23' 42''·294$. The circumstance of the astronomical arc being in *defect* as compared with the geodetical arc, is favourable to the supposition of the discordance being due to the attraction of the mountainous country situate to the north of the arc ; for the ten-

* Phil. Trans., 1855, p. 53, *et seq.*

dency of the disturbing attraction in such a case would manifestly be to produce a greater deviation of the plumb-line at the northern than at the southern extremity of the arc, and thereby to diminish the astronomical determination of the amplitude of the arc somewhat below its true value. The question, however, arises, is the *quantity* of this discordance, namely, 5″·236, the same as that which would be occasioned by the disturbing influence exercised upon the direction of the plumb-line at the two stations? The author has mainly devoted his paper to the discussion of this important point.

The geodetical operations cannot be considered as wholly exempt from the influence of mountain attraction, seeing that in determining the elevations and depressions of the stations of the various triangles, the direction of the plumb-line must be taken into account. The author, however, has shown (what indeed could scarcely have required demonstration) that in consequence of the smallness of the elevations and depressions the effect thus produced is utterly insensible. He next investigates the effect which a small error in the amplitude of an arc would produce on the value of the ellipticity; and he finds, that by assuming the ellipticity to be $\frac{1}{300}$, an error in defect of 5″·236 in the amplitude would, under the most favourable circumstances, diminish the ellipticity by $\frac{1}{25}$th of its whole value. It is not, therefore, sufficient to attribute the discordance to the influence of mountain attraction; but, on the contrary, it becomes an object of the highest importance to ascertain whether the effects due to this cause are capable of accounting exactly for the observed anomaly.

In order to calculate the attraction at any station, A, of the superincumbent matter contained on the earth's surface, the author supposes a series of vertical planes to be drawn through A, making any angles with each other, and dividing the surface through A parallel to the sea-level, into a number of *lunes*, all meeting again in a point at the antipodes of A. He then draws a series of concentric circles upon this surface, having A for their common centre, the distance between two consecutive circles being regulated by a fixed law. In this way he divides the whole surface into a number of four-sided *compartments*, two of the sides in every compartment converging to A, and the other two being parts of circles concentric with respect to A.

Supposing β to be the angle contained between any two vertical planes passing through A, and which consequently measures the breadth of the lune formed by them; α and $\alpha + \varphi$, the angular distances from A of any compartment in this lune; ϱ, the mean density of the earth; and h, the average height of the earth's surface above the level of the compartment; the author finds the attraction upon A of the whole mass standing on the compartment to be

$$= \varrho \sin \tfrac{1}{2} \beta \cdot \frac{\varphi \cos^2 (\tfrac{1}{2}\alpha + \tfrac{1}{4}\varphi)}{\sin (\tfrac{1}{2}\alpha + \tfrac{1}{4}\varphi)} \cdot h$$

This expression is simplified by supposing the relation between α and φ to be such that

$$\frac{\varphi \, \cos^2 \left(\tfrac{1}{3} \alpha + \tfrac{1}{4} \varphi \right)}{\sin \left(\tfrac{1}{3} \alpha + \tfrac{1}{4} \varphi \right)} = \text{a numerical constant} = c$$

The value of c being determined by making $\varphi = \frac{1}{10} \alpha$, φ and α being both supposed indefinitely small, the relation between φ and α is reduced to the following form:

$$\frac{\varphi \, \cos^2 \left(\tfrac{1}{3} \alpha + \tfrac{1}{4} \varphi \right)}{\sin \left(\tfrac{1}{3} \alpha + \tfrac{1}{4} \varphi \right)} = \frac{4}{21}$$

whence attraction of mass standing on any compartment

$$= \frac{4}{21} \, \varrho \sin \tfrac{1}{2} \beta \, . \, h$$

This expression depends upon h, the average height of the surface of the mass, above the surface through A, and not at all upon the distance of the compartment from A.

The equation between φ and α obviously fixes the relation between the distances from A of any two consecutive circles, the arcs of which form the sides of the compartments; or, in other words, the relation between the length of any compartment and its distance from A. The author calls this relation the *Law of Dissection*.

It will be seen that the expression for calculating the attraction of the matter above the surface passing through A is reduced to a formula of extreme simplicity. It only remains to calculate the distances from A by means of the Law of Dissection, and lay down the circles and lines diverging from A upon a good map on which the elevations and depressions are marked, and the attractions of the several masses standing on the compartments thus marked out will be given by the formula for the attraction as soon as their average elevations have been determined.

Assuming the mean density of the earth to be 5·66 times the density of distilled water as determined by Baily, and supposing the density of the superficial crust of the earth to be = 2·75, which is the density assigned to the mountain Schehallien, the author finds the deflection of the plumb-line caused by the mass standing on any one compartment to be

$$= 1''\!\cdot\!1392 \sin \tfrac{1}{2} \beta \, . \, h$$

The equation between φ and α involving the Law of Dissection, not admitting of a direct solution, the author employs an ingenious process by which he is enabled to effect this object with all desirable accuracy, and hence he derives the lengths of the compartments and their respective distances from A. These distances being laid down and the concentric circles drawn upon a map or globe, it only remains to ascertain the average heights of the masses standing on the compartments thus drawn. The author

suggests some practical methods for effecting this object, and con-
cludes the exposition of his method of computation with the follow-
ing remark : —

" It will be evident that mountain ranges will assume a less
importance in this calculation as they are more distant from A,
since they will stand on a larger compartment ; and therefore,
when in imagination levelled down to cover the whole compart-
ment, and give the average height, they will stand at a much less
altitude. It is for this reason that a knowledge of extensive
table-lands of considerable elevation, and of the elevated channels
of rivers is of far more importance in this part of the calculation,
especially in the remoter parts, than of mountain-peaks and
mountain-ridges."

The author next investigates an approximate value of the
intensity of mountain attraction at the two extremities and the
middle station of the arc. The extreme stations are Kaliana, in
latitude 29° 30′ 48″ and Damargida, in latitude 18° 3′ 15″ ; the
middle station is Kalianpur, in latitude 24° 7′ 11″. The northern,
middle, and southern stations are denominated by him respectively
A, B, and C.

He first traces out upon a map the tract of country which
he supposes to exercise a sensible influence upon the direc-
tion of the plumb-line in India. This he calls the *Enclosed
Space.* Before describing the geographical features of this region,
he shows, by reference to Humboldt's *Aspects of Nature* and
other authorities, that the attraction of the countries situate exter-
nally with respect to it cannot sensibly affect the direction of the
plumb-line at any station of the great arc of India. He then
divides the Enclosed Space into two regions, which he denomi-
nates respectively the *Known Region* and the *Doubtful Region.*
The general features of the Known Region are gathered from the
Survey and other maps of India. With respect to the Doubtful
Region the author is compelled to rely upon the statements of
Humboldt and other travellers. Having divided the Enclosed
Space into a number of four-sided compartments by drawing a
series of lines and concentric circles corresponding to each of the
three stations, in accordance with the method of computation
which he had already explained, he endeavours to determine the
average height on each compartment by means of the knowledge
thus derived respecting its geographical features. The compu-
tation of the attraction at each station was then effected with
great facility. The following are the final results at which he
arrived : —

Deflection of plumb-line in meridian at A	..	27·853″
,,	,,	at B	..	11·968
,,	,,	at C	..	6·909
,,	in prime vertical at A	..	16·942
,,	,,	at B	..	4·763
,,	,,	at C	..	2·723

Hence we obtain

> Difference of Meridian Deflections at A and B .. $15 \cdot 885''$
>
>   „   „   at A and C .. $20 \cdot 944$
>
>   „   .   „   at B and C .. $5 \cdot 059$

The first of these quantities is considerably greater than $5'' \cdot 236$, the quantity brought to light by the *India Survey;* and the values of the deflections at B and C bear a far higher ratio to those at A than has been generally supposed. The following are the results for the total deflection at each of the stations : —

> Total Deflection at A .. $32 \cdot 601''$, and in Azimuth, $31° 18'$ east.
>
>   „   „   B .. $12 \cdot 880$ „ 21 42 „
>
>   „   „   C .. $7 \cdot 426$ „ 21 31 „

The author next makes various suppositions for reducing the computed deflection so as to make it agree more nearly with the results of observation. His original assumption was that the density of the elevated region is $2 \cdot 75$ times the density of distilled water, which was the estimated density of the mountain of Schehallien. His opinion is that this assumption is not too great, since a large share of the deflection is produced by the attraction of the elevated plateau which lies in Thibet and south of that country ; and as this is on an average 10,000 feet or more high, the lower part of the materials must be denser rather than lighter than those of a mountain of inconsiderable altitude. " If, however," continues the author, " we do reduce the density, say to $2 \cdot 25$, which is yielding much, still the deflections and their differences are reduced by only one-fifth part, and therefore this will not solve the difficulty."

The author next has recourse to an hypothesis which is tantamount to supposing the whole mass on the Doubtful Region to be non-existent ; but even in this case the difference of deflection at A and B is not reduced below $9'' \cdot 753$, which is greater than $5'' \cdot 236$ in the ratio of 13 to 7. Nor will the attraction be sufficiently diminished if the density of the remaining mass on the Known Region be reduced.

A third means of reduction founded upon the supposition of the non-existence of the mass on the Doubtful Region, and a diminution of the altitude of the mass on the Known Region is found to be equally ineffectual in reconciling the computed with the observed deflection.

By calculation the author finds

$$\frac{\text{Mass on Doubtful Region}}{\text{Mass on Known Region}} = 6 \cdot 264$$

or the mass on the Doubtful Region is greater than that on the Known Region in a ratio higher than 25 to 4. Also,

$$\frac{\text{Mass on whole Enclosed Space}}{\text{Mass of the Earth}} = 0 \cdot 00002825 7$$

The author next computes for each station the point at which the whole of the elevated mass would require to be concentrated in order that it might produce the same amount of deflection. In this manner he finds the

Distance from A of point of concentration　**=** 1688 Miles.

.. 　B　.. 　.. 　**=** 2692

.. 　C　.. 　' 　.. 　**=** 3544

The differences of these two last from the first is far greater than the distance of B and C from A, viz. 5° 23′ 37″ and 11° 27′ 33″. From this it is easily inferred, what indeed did not need this proof, that the mass in no sense whatever, even an approximate one, attracts as if concentrated in a fixed point.

Assuming that the heights of the elevated region have been rightly assigned in his paper, the author finds by a comparison of the northern and southern divisions of the Indian arc, that its ellipticity amounts to $\frac{1}{426\cdot2}$, a result which shows that the arc is more curved than it would be if it had the mean ellipticity. The author illustrates the increased curvature by comparing the height of the middle point of the arc above the chord with the height of the same point furnished by the mean ellipticity $(\frac{1}{300})$ and by the ellipticity which has been deduced from the Indian arc without taking into account the effect of mountain attraction, viz. $\frac{1}{196\cdot3}$. In this way he finds

For the mean ellipticity, height **=** 19·8992 Miles.

Ellipticity $\frac{1}{426\cdot2}$　　19·9290

.. 　$\frac{1}{196\cdot3}$　　19·8460

" The ellipticity, therefore," says the author, " which results from taking account of mountain attraction raises the middle point of the arc by 0·0298 of a mile, or 157 feet; whereas the ellipticity, when mountain attraction is neglected, depresses the arc through 0·0532 of a mile, or 281 feet. These quantities are nearly in the ratio of 5 to 9. Hence the consideration given to mountain attraction in this paper brings the curvature of the Indian arc nearer the mean curvature than the neglect of mountain attraction does in the ratio of 5 to 9. This is, as far as it goes, in favour of these calculations.

Deep River, Cape of Good Hope,
July 12, 1854."

*On the Computation of the Effect of the Attraction of Mountain
Masses as disturbing the Apparent Astronomical Latitude
Stations in Geodetic Surveys.* By G. B. Airy, Esq., Astr
nomer Royal.*

In this short paper the author considered the conclusion
which Archdeacon Pratt was conducted by his investigation of the
effects of mountain attraction on the direction of the plumb-line
India. He remarks that not only is there nothing surprising in the
discordance between the observed and computed deflections, but
that it ought to have been anticipated, and that, instead of expect-
ing a positive effect of attraction of a large mountain mass upon
station at a considerable distance from it, we ought to be prepared
to expect no effect whatever, or in some cases even a small nega-
tive effect.

For the purpose of illustrating his views the author supposes
the crust of the earth to be ten miles thick, and to be of less den-
sity than the fluid in the interior, which for convenience of desig-
nation he calls *lava*. Supposing a mass of table-land to rest upon
the earth's surface, the breadth of which mass in its smaller hori-
zontal dimension is 100 miles, and the height two miles, the author
maintains that the permanent existence of such a state of things is
not possible in nature, for the weight of the table-land would
break the crust through its whole depth from the top of the
table-land to the surface of the lava, and either the whole or only
the middle part would sink into the lava. He proceeds to prove
this by showing that the cohesion which would be necessary to
prevent the table-land from breaking the crust would be capable
of supporting a hanging column of rock twenty miles long, a
conclusion which is contrary to all experience. Other suppositions
with respect to the thickness of the crust are shown to lead to
conclusions equally inadmissible.

The author is of opinion that there can be no other support
to the table-land than that arising from the downward projection
of a portion of the earth's light crust into the dense lava. He
conceives, in fact, that the state of the earth's crust lying upon
the lava may be compared with perfect correctness to the state
of a raft of timber floating upon water, in which, if we remark
one log whose upper surface floats much higher than the upper
surfaces of the others, we are certain that its lower surface lies
deeper in the water than the lower surfaces of the others.
According to this view of the subject the disturbing influence
will depend on the positive attraction produced by the elevated
table-land and the negative attraction produced by the substi-
tution of a certain volume of light crust for heavy lava.

The general conclusion at which the author arrives is this : in
all cases the real disturbance will be less than that found by com-

* Phil. Trans., 1855, p. 101, *et seq.*

puting the effect of the mountains, on the law of gravitation. Near to the elevated country the part which is to be subtracted from the computed effect is a small proportion of the whole. At a distance from the elevated country the part which is to be subtracted is so nearly equal to the whole that the remainder may be neglected as insignificant, even in cases where the attraction of the elevated country itself would be considerable. But in our ignorance of the depth at which the downward immersion of the projecting crust into the lava takes place, we cannot give greater precision to the statement.

The author finally remarks that the condition of *breakage* of the table-lands which pervades his reasoning is not applicable to such elevations as Schehallien, but to high tracts of very great horizontal extent, such as those to the north of India.

Measurements of Saturn made with the Astronomer Royal's Double-Image Micrometer employed with a Thirteen-inch Newtonian Equatoreal. By Warren De la Rue, Esq.

				Reduced to M.D. 9ˑ5430
Outer diameter of Outer Ring,	Oct. 30, 1854	...	45ʺ913 ...	39ʺ63
„	„	Dec. 3,	47ˑ226 ...	39ˑ83
„	„	10,	47ˑ811 ...	40ˑ35
„	„	Dec. 23, 1855	46ˑ970 ...	39ˑ53
			Mean ...	39ˑ83
Inner diameter of Outer Ring,	Oct. 30, 1854	...	41ʺ345 ...	35ʺ69
„	„	Dec. 3,	41ˑ629 ...	35ˑ11
„	„	10,	11ˑ700 ...	35ˑ20
			Mean ...	35ˑ33
Outer diameter of Middle Ring,	Dec. 3, 1854	...	39ʺ352 ...	33ʺ19
„	„	10,	39ˑ950 ...	33ˑ72
			Mean ...	33ˑ45
Inner diameter of Middle Ring,	Dec. 3, 1854	...	31ʺ03 ...	26ʺ17
„	„	18, 1855	32ˑ87 ...	27ˑ65
			Mean ...	26ˑ91
Equatoreal diameter of Planet,	Dec. 3, 1854	...	20ʺ967 ...	17ʺ66

Each of the measures given is the mean of at least six readings of the micrometer, the contacts being made alternately on opposite sides. One revolution of the micrometer being equal to 8ʺˑ018, except with the lens employed on Dec. 10, 1854, when one revolution was equal to 4ʺˑ4001.

After the conclusion of the ordinary business of the meeting, Mr. Main gave a verbal account, illustrated by drawings, of the substance of his paper communicated to the Society on the *Dimensions of the Rings of Saturn.*

The drawings were intended to exhibit to the eye the appearance of the Saturnian system according to the various estimations of the observers who had chiefly devoted themselves to the scrutiny of the planet, namely, M. Encke, Mr. De la Rue, Capt. Jacob, Mr. Main, and M. Otto Struve, for the purpose of enabling the members of the Society, who were furnished with good telescopes, and who had been in the habit of viewing *Saturn*, to judge of the probable fidelity of the different estimations. Of these representations Mr. Main's was that which gave the largest measure of the dark interval between the body of the planet and the inner edge of the interior bright ring, while M. Otto Struve's representation gave by far the smallest value. Mr. Main drew attention to this fact, that the measures of M. Otto Struve were substantiated by those of no modern or contemporaneous observer, and were totally at variance with all the ancient observations, and consequently that the natural inference was, that some unusual circumstance had affected his estimations. He also showed that if M. Otto Struve's results were neglected, there would be no ground whatever for his theory respecting the approximation of the rings towards the body of the planet. In connexion with this part of the subject, Mr. Main drew attention to a Dutch pamphlet recently written by Professor Kaiser of Leyden,* in which, by considerable research, new light was thrown upon the observations of Huygens, and, at least during the lifetime of that observer, the inferences drawn by M. Otto Struve were found, by additional observations of Huygens, to be contradicted.

Mr. Main then drew attention to a fact connected with his own measures, as showing the delicate nature of such inquiries, and the singular errors to which the measurers are liable. In the early portion of the measures of 1853, for instance, the measured breadth of the dark space was larger than the breadth of the system of bright rings; this difference between them decreased continually and systematically till the two appeared to be equal, after which the breadth of the dark rings became increasingly larger than the dark space. This difficulty was ultimately cleared up by a comparison of the values of the diameter of the ball of the planet obtained in this series of measures, with Mr. Main's own standard value given in his paper on the *Form of the Planet Saturn*, by which it appeared that the excess of the standard above the measured values decreased continually, and that when these excesses were applied negatively, as corrections to the mea-

* Professor Kaiser has recently informed the Editor, in reply to a communication respecting this paper, that it is also published in vol. iii. of the *Proceedings of the Royal Academy of Sciences of Amsterdam.* An account of it will be given in a future number of the *Notices.*—R. G.

sured breadths of the dark space, the anomaly entirely disappeared.

In confirmation of Mr. Main's measures, he stated that he had received from the President, Mr. Johnson, some very recently-made measures, kindly undertaken at his request, which agree very closely with his own ; and the announcement of this agreement elicited from the members present an expression of approbation.

A conversation on the subject afterwards followed, in which the President, the Astronomer Royal, Mr. Hind, and Mr. De la Rue took part, and the general correctness of Mr. Main's conclusions seemed to be acquiesced in by all those gentlemen.

Elements of Atalanta. By M. Bruhns.

Epoch 1855, Nov. 0. Berlin M.T.

$$
\begin{array}{llrrr}
 & & \circ & \prime & \prime\prime \\
M & \ldots & 341 & 22 & 48\cdot1 \\
\varpi & \ldots & 40 & 42 & 12\cdot7 \\
\Omega & \ldots & 359 & 0 & 8\cdot8 \\
i & \ldots & 19 & 6 & 45\cdot2 \\
\varphi & \ldots & 17 & 5 & 3\cdot5 \\
\mu & \ldots & & & 769''\cdot261 \\
\text{Log.}\,a & \ldots & & & 0\cdot442621
\end{array}
$$

These elements are calculated from the following three Berlin observations :

		M.T. Berlin.	R.A.	Decl.
		h m s	° ′ ″	° ′ ″
1855.	Oct. 12 ..	10 33 3·9 ..	343 50 32·0 ..	6 48 49
	17 ..	9 46 31·4 ..	343 1 43·8 ..	6 3 15·6
	23 ..	7 4 27·4 ..	342 21 43·8 ..	5 6 16·4

Observations of Comet III. 1855, *taken with the* $8\frac{1}{2}$*-inch Refractor of the Liverpool Observatory.* By John Hartnup, Esq.

	G.M.T.	Comet's R.A.	Log. $\frac{p}{P}$	N.P.D.	Log. $\frac{q}{P}$
1855.	h m s	h m s		° ′ ″	
Dec 2	11 57 9·5	7 4 11·13	−8·395	86 24 55·2	−9·8856
2	12 54 1·0	7 3 10·76	−8·210	86 24 19·7	−9·8828

Star of Comparison, Procyon.

Assumed mean place for Jan. 0, 1855.

7^h 31^m 42^s·51 R.A.   84° 24′ 24″·45 N.P.D.

M. Brorsen has communicated to the *Astronomische Nach-richten* the following note respecting the phenomenon accom-panying the zodiacal light, alluded to in the *Monthly Notices* for November:—

"I have observed this phenomenon here, and also upon the island of Alsen in the duchy of Schleswig, during the last two years, and have given a detailed account of it in Dr. Jahn's *Weekly Conversations on Astronomy* for 1854. The observa-tions, which essentially agree in both years, are the following:—The phenomenon is seen, not only about the time of the vernal equinox, but also during the period of the autumnal equinox, although undoubtedly it is more conspicuous in the earlier season of the year. The first faint traces of it are perceived in the month of February. It continues to increase in brightness and extent throughout the months of March, April, and the beginning of May. The fainter and less considerable phenomenon of autumn is visible during the months of September, October, and Novem-ber. With respect to both phenomena, I have convinced myself by repeated observations, that the brightest part lies exactly opposite to the sun, so that the estimated point of most intense light falls decidedly within a degree of the point which is in opposition to the sun. The observations further indicate, that the phenomenon of spring connects itself with the western zodiacal light towards the middle of April by a faint streak of light, gradually increasing in brightness, which was not previously visible. But the phenomenon of autumn appears in the first half of November to extend along the ecliptic as far as the western horizon in the shape of a very faint and previously invisible zone of light, which gradually increases in brightness and in the breadth of its base, until at length it assumes the well-known aspect of the winter evening zodiacal light. From this time, however, till the beginning of March, its extremity remains almost stationary about the region of τ_1 and τ_2 *Arietis*. Both phenomena can be seen here distinctly every evening at the same season of the year, when the sky is clear, and the observed position of the heavens is moderately high; nay, they are even visible at the commencement of moonlight.

"*Senftenberg, Nov.* 13, 1855."

At the meeting of the Society there was exhibited a series of beautiful photographs of the moon, which were executed with the great refractor of the Cambridge Observatory, U.S.

Mr. A. F. M'Intosh has forwarded a paper on *Saturn's* rings. He supposes that these appendages originally consisted of separate streams of a fluid substance circulating around the sun in the vicinity of *Saturn's* orbit; that the planet's attraction converted them into rings about himself; and that the number of rings is owing to the number of separate streams of matter. The author has entered into various speculations in support of his views, but it would be unnecessary to make any further allusion to them here.

Tafeln der Flora mit Berücksichtigung der Störungen durch Jupiter und Saturn entworfen von F. Brünnow, Dr., Director der Sternwarte zu Ann-Arbor in Michigan. Berlin, 1855.

These Tables of *Flora* are constructed by Dr. Brünnow according to a theory of general perturbations devised by Professor Encke. An account of this theory is given by Professor Encke in a letter to the Astronomer Royal, which is published in the *Monthly Notices* for May 1853. The mean elliptic elements of the planet having been deduced from a series of observations extending only over four years, it cannot be expected that the Tables will be capable of representing the motion of the planet with anything more than an approximate degree of accuracy beyond a limited period of time. The author, however, expresses a confident hope, that at some future epoch, when an extensive series of oppositions of the planet shall have been obtained, the mean elliptic elements may be deduced by the aid of the Tables with such accuracy, that when the perturbations have been strictly computed, new tables of the planet may be constructed, which shall satisfy the wants of the astronomer for a long period of time—perhaps for hundreds of years.

A gentleman engaged in astronomical pursuits is desirous of purchasing a second-hand transit-instrument of modern construction possessing something like the following requirements :—Size, 2 or 2½ feet ; aperture of object-glass, not less than 2½ inches ; portable iron stand ; maker, Troughton and Simms, or Dollond; furnished with one or more moveable wire-micrometers. For further information apply at the Apartments of the Society.

CONTENTS.

ROYAL ASTRONOMICAL SOCIETY.

MANUEL J. JOHNSON, Esq. President, in the Chair.

The Rev. Wm. Russell Almond, Stapleford, near Derby, and
The Rev. George Venables, St. Paul's, Chatham,
were balloted for and duly elected Fellows of the Society.

Volume XXIV. of the *Memoirs* has just been published.
Besides the usual matter, it contains an Index to the last fourteen
volumes. The price is 7s. 6d. to Fellows of the Society, and 12s.
to the public. Vol. XV. of the *Monthly Notices*, which contains
shorter papers, abstracts of memoirs, and notices of various astro-
nomical publications, is given to purchasers of the 4to. volume.
The two publications are supplementary to each other, and are to
be considered as parts of the same series. They contain scarcely
anything in common, and between include a complete account of
the proceedings of the Society during the year.

New Planet.

On the evening of the 12th of January, at $9^h 33^m$, a new planet
of the 9.10th magnitude was discovered at the Imperial Observa-
tory, Paris, by M. Chacornac. The following positions of it were
obtained :—

	Paris M.T.	R.A.	Decl.	No. of Comps.
	h m s	m s	′ ″	
Jan. 12	11 52 43	......	δ ✳ −2 11·9	3
	12 18 22	α ✳ +4 36·7	......	2
13	9 54 32	α ✳ +3 46·9	......	4
	10 21 6	......	δ ✳ −2 37·6	3
	10 43 4	α ✳ +3 45·3	......	2

Position of the star of comparison (8th magnitude), according
to a meridian observation of the 13th :—

R.A. = $8^h 35^m 19^s·99$ Decl. = $+17° 23' 53''·5$

On the Rings of Saturn. By Professor Secchi.

(*Communicated by the Astronomer Royal.*)

" In an interesting memoir published in the *Proceedings of the Academy of Sciences of St. Petersburg*, vol. v. 6th series, 1852, M. Otto Struve has announced the suspicion that the rings of *Saturn* were in a state of gradual collapse, so that, after a certain interval of time, which probably might not be long, the planet would appear totally deprived of those interesting appendages. The suspicion is the more reasonable, inasmuch as nothing proves that the system is in a condition of permanent equilibrium, and since, not only the early observations, which might be liable to some doubt, but even the most recent observations, support the hypothesis. I extract from the memoir of M. Otto Struve the following table in support of this assertion, which will be found useful hereafter. I add to it the measures of Mr. Lassell for 1853 (*Astronomische Nachrichten*, No. 856):—

Table (A).

Observer.	Epoch.	Exterior Diameter of the Ring.	Diameter of the Planet.
Huyghens	1657	45	18
Cassini	1691	45	...
Pound	1719	42	18
Bradley	1719	41·25	17·75
Rochon	1777	40·6	16·9
W. Herschel	1791	46·68	...
W. Struve	1826	40·10	17·99
Bessel	1831	39·30	17·05
Encke	1837	40·93	17·68
Galle	1838	40·90	17·91
O. Struve	1851	39·74	17·61
Lassell	1853	40·88	17·45

" The discordances remarked in these measures are sufficiently sensible, and I have thought that it would not partake of any rashness on my part to attack the question. After having acquired sufficient practice in measures of the planets with *Jupiter*, and having found my measures agreeing with those of M. Struve, I have availed myself of the last opposition and of the maximum opening of the ring, to execute a series of measures, not only of the ring, but also of the planet, so as to ascertain the degree of precision and confidence to which they might be entitled. Each number is the mean of two, and more frequently of three, double measures, which are very accordant, and of which the probable error of an individual measure does not exceed $0''\cdot2$. The measures have been executed only under an excellent condition of the atmosphere, except on two occasions, one of which was for the purpose of ascertaining the influence of less favourable circumstances. To the measures made during the last opposition I have

:o added, for the purpose of comparison, those of the opposition 1854-5. I have not corrected any of the measures for refrac-n nor for phase,— not that I considered the effects depending on these causes to be insensible,— but because I perceived disrdances of another kind of far greater magnitude, the origin of 1ich I was desirous of examining before I calculated those small rrections.

Table (B).

ys of serva-ion.	Exterior Diameter of Exterior Ring.	Middle of Principal Division.	Interior Diameter of Ring B.	Interior Edge of Nebulous Ring.	Equatoreal Diameter of Planet.	Transverse Diameter of Ring.
1854.	″					
ov. 15	40·655	...	...	...	...	...
ec. 15	41·331	...	...	...	...	...
17	41·008	...	...	...	...	...
1855.						
an. 4	40·739	...	...	...	...	...
6	40·733	...	...	...	...	...
pr. 20	41·205	...	...	...	17·708	...
ov. 30	40·851	...	23·792	21·232	17·458	18·339
ec. 5	41·324	34·486	26·101	20·995	17·829	...
14	41·068	...	25·474	...	17·531	20·442
15	41·443	34·657	25·913	21·725	17·773	19·025
16	40·812	34·699	25·834	21·350	17·716	18·504
23	41·118	34·642	25·917	21·605	17·611	18·991
24	40·564	34·760	26·191	21·519	17·687	18·291
27	40·412	...	25·832	21·508	17·572	18·110
	40·623	...	26·003	...	...	...
30	40·710	...	...	...	17·728	...
	41·090	...	26·083	...	...	...
1856.						
an. 9	40·483	...	...	...	...	...
Mean	40·893	34·659	25·714	21·419	17·661	18·814

" All the measures have been reduced to the mean distance of *aturn* from the sun, but the results have not been corrected for ιe different inclination of the ring on the different days. It will ɔ seen by this table that the discordances in the measures of the ng are very considerable, while, in the case of the planet, they ·e sufficiently small: for the ring they are generally not less than ᛁ·5, while the extremes amount to as much as 1″, which is alto-ɟther intolerable with this instrument. After spending some me in anxious reflection upon the cause of these anomalous ᛁsults, I consulted the measures of Mr. Lassell, and in these I ιund similar discordances. This circumstance induced me to iᛁcuss the measures with greater care, and the result has been ιat I think I have detected a certain period in the irregularities. ᛁhe measures of two consecutive days do not agree; but those of ιree days and of nine are consistent. The measures of four days ɔ not agree even in the case of Mr. Lassell's observations. I wᛁs

then led to inquire whether the results might not be explained by
supposing the ring to be elliptic, and that by its rotatory motion
around its primary, it presented alternately its major and minor axis.
Having assumed the period of rotation assigned by Herschel, the re-
sults did not combine; but upon trying the period which a satellite
would have if it were placed on the margin of the ring, I found, after
applying a slight correction, that it satisfied the observations. Let
T be the time of rotation of the ring; t, the time which elapses after
an observed epoch of maximum; x, the difference of the two axes of
the ring; we shall then have the mean diameter = the observed
diameter $+ x \cos 2 \left(\dfrac{2 \pi t}{T}\right)$. If we denote by ω the angle which
remains after whole circumferences have been described by the
satellite, the correction will be $c = x \cos 2 \omega$,[*] and it is evident
that in a rotation there are two maxima and two minima. The
value of T, which satisfies the observations best, is $T = 14^{h}\cdot428$
of sidereal time. From the difference between the maxima and
the minima I have concluded that $x = 0''\cdot366$, and I have chosen
for the epoch of minimum the 24th of December, at $4^{h} 10^{m}$ sidereal
time, on which occasion the planet was so steady and well defined,
that I entertain no doubt respecting the observation. The Table
(C) gives the results of these calculations :—

Table (C).

Date and Hour.		Intervals		Correction.	Diameter		Deviation from Mean
		In Time.	In Revolution.		Observed.	Corrected.	$c - m$.
1855.	h	h m	r o		"	"	
Nov. 30	2·30	24+1 40	40¼+23	+0 254	40·851	41·105	+0·118
Dec. 5	1·45	19+2 35	32 +76	—0 323	41·324	41·001	+0·014
14	3·00	10+1 40	17 +24	+0 245	41·068	41·311	+0·227
15	3·30	9+0 40	15 +81	—0 348	41·443	41·095	+0·108
16	3·30	8+0 40	13½+12	+0 334	40·812	41·146	+0·159
23	4·10	1+0 0	1½+65	—0 235	41·118	40·883	—0·104
24	4·10	...	...	+0 366	40·564	40·930	—0·057
27	3·40	3—0 20	5 +12	+0 334	40·412	40·746	—0·241
,,	4·10	3 0 0	5 +20	+0 280	40·623	40·903	—0·084
30	3·50	6—0 20	10 +32	+0 160	40·710	40·870	—0·117
,,	4·20	6+0 10	10 +45	0 000	41·090	41·090	+0·108
1856. Jan. 9	5·28	16+1 18	27¼+20	+0 280	40·483	40·763	—0·224

Mean .. 40·987

" The column of corrected diameters presents a satisfactory
agreement throughout a period, including forty revolutions of the
ring. The outstanding differences are sufficiently small and of the
order of the errors of observation, although there is a tendency to
a positive excess in the earlier part. The correction for refraction
was not, indeed, applied; and as the hour-angle frequently amounted
to between 2^{h} and 3^{h}, some hundredths of a second might be gained

* The powers of the excentricity, which are higher than the second, are not
taken into account.

in this way, but this would not suffice to make the differences altogether vanish. On the contrary, the excess observable may have the same origin as the anomalies which have characterised the measures of the celebrated astronomers who have formerly devoted their attention to this subject, that is to say, they may arise from real variations of the ring. It will be seen by the above measures that the means of the results of Tables (B) and (C) are not very different from those obtained by MM. Encke, Galle, and Lassell, but deviate from those of MM. W. Struve, Bessel, and Otto Struve, to an extent which far surpasses the probable limit of the errors of observation. It will then be desirable, in order to detect these variations, to compare the breadth of the actual rings with that which has been assigned by M. W. Struve. The mean obtained by M. Otto Struve (memoir above cited) from the observations of MM. W. Struve and Encke and those executed by himself is the following : — breadth of the exterior ring = $2''\cdot440$. From our measures we have determined the breadth of the ring to be $2''\cdot911$, taking into account the breadth of the division which has been found = $0''\cdot402$; the difference, therefore, amounts to $0''\cdot475$ (a).* On the 29th of December I was struck with the excessive apparent breadth of the exterior ring, which appeared to the eye broader than in the incomparable drawings of Lassell, Dawes, Otto Struve, and others. I was induced, therefore, to have recourse to actual measurement, and I found the total breadth from the

Exterior Border of A to the Interior Border of B to be = $7\overset{\prime}{\cdot}512$

Also Breadth of A	= $2\cdot788$
According to Struve	= $2\cdot440$
Difference ...	$0\cdot348$

" The difference is sufficiently great to confirm what simple inspection had already led me to suspect.

" On the 27th of December the state of the atmosphere was very favourable. The two ansæ of the rings were measured. For the preceding ansa we found,—

Double Breadth, including the wires = $1\overset{r}{\cdot}178$ Reduced Breadth.

—   —   —   — $1\cdot178$ $\Big\}$ $8''\cdot6055$

—   —   —   — $1\cdot177$

(Revolution of the Screw = $15''\cdot4667$)

" For the following ansa,—

—   —   —   — $1\cdot196$

—   —   —   — $1\cdot197$ $\Big\}$ $8'''\cdot7506$

—   —   —   — $1\cdot197$

According to Otto Struve $7''\cdot15$

* The third column in Table (B) gives the diameter of the middle of this division (a), upon which the middle of the wire was placed.

" The agreement of the individual measures would not justify us in attributing the difference to errors of observation ; but without having recourse to any exaggeration, it may be seen that the ansæ are somewhat unequal, and that the breadth of the rings was greater than usual. From the above measures (Table B) it results that the interior diameter of the exterior ring A $= 35''\cdot061$; W. Struve makes it $= 35''\cdot289$; the difference is $0''\cdot228$; a quantity which is tolerable, but cannot be neglected in an object like this, which admits of being seen with such precision and facility, under a favourable condition of the atmosphere. It would follow, therefore, that the division has only in a slight degree shifted its position. If, then, there exists any variation, it must be in the breadth of the exterior ring. The ratio of the breadths of the two rings ought, consequently, to vary ; and this circumstance might explain the discordance between the indications of Cassini and succeeding astronomers, in regard to the value of that ratio. In like manner the ratio of the dark space between the ring and the planet to the breadth of the ring ought to vary. The ratio of the former of these two quantities to the latter is,—

According to Otto Struve	...	...	...	0·49
Encke and Galle	...	...	...	0·57
W. Struve	...	...	...	0·64
Bradley	...	...	...	0·95
According to my last measures		...	...	0·53

" We should, then, be disposed to conclude that this system is subject to sensible variations. I should have wished, for this purpose, to wait for new observations ; but I have thought that I ought no longer to delay communicating these results to astronomers, in the hope of engaging them to take exact measures of the ring while the opening has attained its maximum value; for it is to be feared that when the ellipse becomes more acute at the summit, the difficulties will become greater, and the results will be diminished. I offer the foregoing views merely as an essay which future observations may confirm or refute. It will be important henceforward to note exactly the time of observation and to indicate it along with the results obtained. The want of this indication has prevented me from profiting by several interesting observations. The practice of noting the time simply in so far as it is necessary for ascertaining the correction for refraction, is no longer sufficient.

" I shall conclude this paper by extracting from my Journal some observations relative to the appearance of the planet :—

" *24th December.* Atmosphere magnificent; charming image; the exterior ring A exhibits two well-defined streaks, and is, as it were, divided into three rings. The distance of the principal streak from the interior border of A being measured, was found equal to $1''\cdot596$. The second streak is very fine, and is distant

from the interior border of A at the utmost, o″·5. These two
streaks are seen not only at the ansæ, but all round the perimeter,
except above the planet, where they are somewhat confused. The
principal division *is not black*, but of the colour of the nebulous
ring, C, that is to say, somewhat reddish. On other occasions it
appeared bluish, indicating thereby changes of colour. The divi-
sion is very nearly, but not quite so broad at the ansæ, as the
wire of the micrometer (that is to say, nearly o″·402). The differ-
ence of colour may be distinguished very well from the contrast
of the two small points of black shadow which are seen on both
sides of the summit of the planet, and of which one is broader than
the other. The pole of the planet almost touches the ring A by a
veritable mathematical contact. The ring B is seen with its light
graduated, and somewhat brighter towards the interior edge.
The curve here exhibited gives the approximate degree of the in-
tensity of the light of the rings. The division between B and C is

| A | B | C |
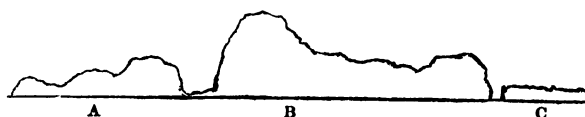
seen well defined; its breadth equal to half the principal division.
The ball of the planet presents a very brilliant band. Upon care-
fully examining the curvature of the lower margin of this band,
we see that *it has not exactly the continuation of the nebulous ring;*
so that the latter traverses the planet in a different part, and is
projected beneath the bright band, and upon a more obscure zone.
The pole of the planet is greenish grey, magnifying powers, 1000
and 500.

"*27th December*. The nebulous ring very well defined: more
bluish than usual. *Enceladus* is very well seen in the field with-
out making the planet go out of it. It is recognised by means of
the position which it occupied in the preceding day. The globe
scarcely touches the ring A; magnifying power, 1000 and 700.
The principal division *is not black*.

"*31st December*. The globe exhibits a very small black fila-
ment under the border of A. This is verified by M. Galli (this
young man has an excellent sight; he sees habitually, besides the
seven stars of the *Pleiades*, three others the positions of which I
have verified by means of the map). However, it is far from
being under all the division. I would value this zone at one-
eighth of a second. The shadow laterally is sufficiently developed,
but it presents a protuberance at the summit (see the annexed
figure). The nebulous ring
well defined, but it traverses
the planet in a part different
from the inferior limit of the
bright zone. (Upon observing at a distance with the telescope a

globe of crystal, in which is contained a jet of illuminating gas: the tinge of the crystal resembles that of the nebulous ring.)

"11*th January*, 1856. The two ansæ of the ring C are of different shades. This, on the side of the shadow, is blue; the other, reddish. The eye-pieces are achromatic, and have been changed expressly for that purpose."

On a new Variable Star. By J. R. Hind, Esq.

" On the evening of December 15th, 1855, I remarked in R.A. (1856) 7^h 46^m $33^s\cdot65$, N.P.D. $67°$ $37'$ $17''\cdot1$, an object shining as a star of the ninth magnitude, with a very blue planetary light, which I have never seen before during the five years that my attention has been directed to this quarter of the heavens. On the next fine night, Dec. 18, it was certainly fainter than on the 15th by half a magnitude or more. Since that date I have not had an opportunity of examining it till last evening, January 10th, when its brightness was not greater than that of stars of the twelfth magnitude. It is evidently a variable star of a very interesting description, inasmuch as the minimum brightness appears to extend over a great part of the whole period, contrary to what happens with *Algol* and *S Cancri*.

" The position given above was deduced by micrometrical comparisons with the principal component of the double star Σ 1158. The *variable* precedes 1^m $26^s\cdot53$, and is N. $7'$ $30''\cdot8$.

" *Mr. Bishop's Observatory*, 1856, *January* 11."

On the Occultations of the Star Antares (α *Scorpii*) in the Year 1856. By G. B. Airy, Esq., Astronomer Royal.

" In the year 1856 there will be several occultations of *Antares* by the moon visible in England, namely, on March 26, June 16, and August 10.

" It appears desirable that advantage should be taken of these phenomena for observation of the double character of the star.

"*Antares* is the only close double star of the first magnitude. Its duplicity was discovered, so far as I am aware, by Professor Mitchell, of Cincinnati, on the 13th of June, 1846,[*] with the large Munich telescope, mounted equatoreally in the Observatory there.

" After the double character of the star had been discovered by direct observation, it was remembered that, in the accounts of observed occultations of *Antares* by the moon, it had been remarked that the light of the star did not disappear or reappear entirely at one instant, but at two instants, with such a sudden change or *saltus* in the character of the light as made it probable that the small companion was seen for a time, while the large star was hidden by the moon.

[*] Silliman's *American Journal* (New Series), vol. i. p. 315.

" The small elevation of *Antares*, even when on the meridian, and the consequent confusion of image, make it impossible for us to see it as a double star in these latitudes. But the sudden change in the intensity of its light at occultation may probably be observed; and a careful register of the times of change of brightness and total disappearance, or of first reappearance and full brilliancy, will give elements for a computation of the relative position and distance of the two stars.

" *Royal Observatory, 1856, January 1.*"

Observations of the Planet Saturn. By William Lassell, Esq.

" 1855, *Nov. 27th.* 10ʰ 30ᵐ G.M. T. Viewed *Saturn* with powers 430 and 565. Sky hazy, with hoar-frost. Planet continually disturbed by passing haze, and observation very tantalising. The shadow of the ball is seen all along the southern limb, extending into the division of the ring, and is lost in it. The ball evidently does not itself extend up to the division of the ring. The shadow seems quite flat-topped, conveying the idea that its presumed roundness is lost in the division; and if the extreme south limb of the shadow does really extend over the division, it is so faint as not to be distinguishable on the darker ring A.

" *Nov. 29th.* *Saturn*, about 11 P.M. was surveyed some time, but the air was worse than on the 27th. My impressions of the form and position of the shadow of the ball and the encroachment of the latter on the ring, are the same as on that occasion. On the 27th I noticed that the preceding arm of C was brown and the following grey.

" *Dec. 6th.* The division of the ring with power 565 seems quite uninterrupted all round. At the southern part the shadow of the ball comes up to it, and is lost in it; but, indeed, the southern limb of the planet becomes gradually so dark, that at its very edge it cannot be distinguished from shadow; and it is only by completing in imagination the symmetry of the ball in that part that an opinion can be formed, whether it touches the division itself, or whether there is an exceedingly narrow portion of shadow between. The sort of osculation which takes place between the shadow and the division of the ring, or between the dusky limb of the ball and the division of the ring, analogous to the *cornering* of the shadow I observed at Malta, is still plainly visible, but is to me without satisfactory explanation.

" *Dec. 12th.* Similar observations to the above were repeated.

" *Dec. 21st.* Circumstances rather unusually fine. Viewed *Saturn* about 10 P.M. with power 565. There is a very slight cornering of the shadow (?) at the south limb. The extreme south limb of the planet touches, I believe, the division. I cannot see

that the shadow is visible there beyond the limb, and there is certainly no encroachment of either shadow or ball upon ring A. Indeed, from the present greater elevation of the sun than of the earth above the plane of the ring, there ought to be no shadow visible. The dark ring is remarkably well shown, and certainly reaches fully half-way between the inner edge of B and the planet's limb. It is well defined at its interior edge, and the interior edge also of B is very sharp. The ruddy belt on the planet is striking, fading, however, very much away towards· the limbs, which are well seen through the dark ring. The brownish yellow of the ball generally, south of the belt, and the very dark south pole, are strongly marked. The aperture of the telescope was reduced to 20 in., but the light was sensibly less, without a greater precision of image. Now that the ring is so widely open, the distance from the inner edge of B to the planet seems less than my drawings, made in October 1852 at Malta, would indicate, in proportion to the breadths of the bright rings. The bit of sky on each side of the ball is well contrasted in blackness with the dark ring, notwithstanding the proximity of the almost full moon.

"1856, *Jan. 7th.*   10 P.M. Wind east, light, and the sky very variable; generally, indeed, cloudy, but with momentary glimpses of good vision, though I doubt if the atmosphere was ever really clear. Viewed *Saturn* for some time with powers 430 and 565, watching for the finest glimpses. I am satisfied there is no shadow visible south of the southern pole, and that the south pole enters the division of the ring, passing, indeed, nearly, if not quite, through it. I receive the impression of a flattening of the ball there, or as if a bit of it were sliced off. I do not see that the ball reaches at all beyond the division, and I should best represent my conviction by saying that the southern edges of the ball and division were coincident. The shadow of the ball upon the ring is

plainly seen as a narrow black line south, following the ball, principally upon the ring B, not extending to A, and faintly seen, or imagined to be seen, across C.

"The division of the ring, generally termed black, is, however, of a much less intense shade of blackness than the shadow of the ball and the portion of sky within ring C, which are both preeminently and intensely black, whilst the division, in comparison with them, may be termed a brown or greyish black. The angular shape of the shadow, where it touches the division, remains as in former apparitions, and also something like a notch, even on the preceding side, where there is no shadow visible. Observation very troublesome, by reason of the haze or scud constantly passing and reducing the brightness of the planet down to that of a star of the fifth magnitude; and, indeed, frequently rendering it invisible to the naked eye.

"These observations, especially touching the encroachment of

the ball upon the ring, have been made in some measure to satisfy
the query of Professor Secchi, in his paper in No. 982 of the
Astronomische Nachrichten. They do not appear to afford any
evidence of oscillation of the plane of the ring. Neither have I
been able to detect any want of symmetry in the form of the di-
vision. But the sky during the latter part of the autumn and
winter has been remarkably unfavourable for delicate scrutiny.

" In addition to the above observations of *Saturn,* I may
mention a

Remarkable Configuration of Stars

found on pointing the telescope in the neighbourhood of *Uranus.*
Approximate place, hastily and rather rudely taken, R.A. 3^h 13^m,
P.D. 72° 50′. No opportunity was afforded of measuring any of
the stars, or even correctly ascertaining their magnitudes. They
appeared thus in the field of view with power 260:—

" The four principal stars are very
bright, and of very nearly equal mag-
nitude. The companions of the three
forming the equilateral triangle are
very minute, so as to form similar
and extremely unequal double stars.
The south following companion is a
delicate object even with 24 inches'
aperture. Some relation amongst them
is suggested by their remarkable juxta-
position.
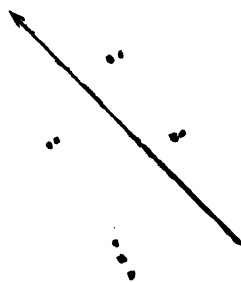
" *Bradstones, 9th January,* 1856."

On Occultations of Stars by Saturn. By M. Winnecke.

(*Extract of a Letter to the Astronomer Royal.*)

" You will excuse my troubling you with a remark on the
subject of your note in the December Number of the *Monthly
Notices,* relative to the occultation of a star by *Saturn,* which was
pointed out in No. 920 of the *Astronomische Nachrichten.* I
hardly think that the conjunction will result in a real occulta-
tion, since the ephemeris of *Saturn,* published in the *Nau-
tical Almanac,* has given the declinations in recent years with far
greater accuracy than the astronomical ephemeris of this place
has done. Assuming the *Nautical Almanac* as the basis of com-
putation, we find the geocentric declination of the centre of *Saturn*
for the instant of conjunction in right ascension with the star 7½^m
to be : —

$$\delta\;\mathbin{\vcenter{\hbox{$\hbar$}}} = + 22° 14′ 6″$$

" The American Ephemeris gives nearly the same value ;—

$$\text{☽ ♄} = +22° \ 14' \ 9''$$

" The apparent declination of Lalande 13545 is, however,—

$$\text{☽ ✳} = +22° \ 14' \ 26'' \ \text{(Bessel's Zone 279, Lal.)}$$

" So that, probably, there will be only a close approach to an occultation. On the 13th of November, 1854, the sky was clear here. *Saturn*, as might be expected from the meridian observations of M. Bruhns (*Ast. Nach.* 922), remained to the south of Lal. 9362, without occulting it. It was very striking, however, on the occasion of this near conjunction to remark the faint light of this star of the 7.8 magnitude, compared with the light of *Titan*, which, it must be admitted, was farther removed from the planet. The star shone so feebly that it seems doubtful whether, apart from all other considerations, a star of the eighth magnitude would, from this circumstance alone, be visible in the space between the ring and the body of the planet.

" By extending my researches to the next year I have arrived at the remarkable conclusion, that, on the 29th of June, 1857, *Saturn* will, in all probability, almost centrally occult *☽ Geminorum*, a star of the third magnitude. If we assume the apparent position of the star for this day to be,—

$$\alpha = 7^\text{h} \ 11^\text{m} \ 35^\text{s}\cdot 20 \qquad \text{☽} = +22° \ 14' \ 37''\cdot 0$$

the different ephemerides assign for the moment of conjunction in right ascension,—

Berliner Jahrbuch, 1857, June 29, 11$^\text{h}$ 47$^\text{m}$ G.M.T. Geoc. Dec. ♄ = +22° 14′ 48′·0
Nautical Almanac   ...   11 53   ...   ...   +22 14 34·5
American Ephemeris   ...   12 3   ...   ...   +22 14 37·9

The Polar Semi-diameter of *Saturn* is 7″·9 (American Ephemeris).

" It is, indeed, very much to be regretted that a phenomenon of such rare occurrence, and one which might furnish important indications relative to the nature of the divisions of the ring, the transparency of the obscure ring and the perhaps oscillatory dimensions of the Saturnian system, can hardly be well observed. The position of the sun for the instant of conjunction is, in fact, $\alpha = 6^\text{h} \ 35^\text{m}$, $\text{☽} = +23° \ 13'$. Notwithstanding this unfavourable circumstance, it may be hoped that in consequence of the brightness of the star, this phenomenon, although invisible to us, may be observed in the daytime by the aid of the powerful telescopes of the Observatories of Cambridge, U.S., and Washington.

" The rarity of the phenomenon is still further enhanced by the circumstance that *☽ Geminorum* is a double star. If we regard the observation of the elder Herschel as probably erroneous, it will be found that the remaining measures do not exhibit any pronounced relative movement.

" Struve's data for the star are : —

Magnitude 3^m, 8^m·2   Dist. = 7″·13   Position Angle 197° 39′

whence it follows that the companion star precedes the principal star by 2″·2, and is farther south than it by 6″·8. If we now assume that the place given in the *Nautical Almanac* is exact, it results that the centre of *Saturn* will pass *between* both stars. Galle has found by calculations based on the doctrine of probabilities, that, taking into consideration all the stars down to the ninth magnitude, inclusive, *Saturn* will occult one of such stars only once in every 181 years. If we now assume, in round numbers, the number of these stars to be 200,000 (Humboldt, *Cosmos*, Band iii. s. 146), which will not be far from the truth, and estimate, according to the same authority, the number of stars included between the first and third magnitudes to be 275, it follows, upon the supposition of an uniform distribution of stars in the celestial vault, that *Saturn* occults a star of one of these three classes of magnitude only once in 130,000 years.

" The above number assigned by Galle is, however, liable to some objections. I have not quite at hand the work in which it is deduced, but I rather think that it holds good only for an observer situate on the sun ; whence it would follow that if we consider an inhabitant of the earth, the number must be diminished ; that is to say, the occultations by *Saturn* must be more abundant. At all events, many thousands of years will elapse before an equally bright star will be again occulted by *Saturn*, and more especially a double star.

" If this matter should appear of sufficient importance to you, I beg of you to have the goodness to communicate it to the Royal Astronomical Society.

" *Berlin, January 28, 1856.*"

Places resulting from Observations of Recently Discovered Small Planets made with the Transit Circle, at the Royal Observatory, Greenwich, during the month of December 1855.

Iris.

Greenwich M.S. Time.	R.A.	N.P.D.
h m s	h m s	° ′ ″
1855, Dec. 6 13 2 14·9	6 3 13·06	67 3 48·45
12 12 32 26·7	5 56 59·33	67 31 26·82
13 12 27 26·2	5 55 54·63	67 36 6·41
18 12 2 20·6	5 50 27·62	67 59 25·31
21 11 47 19·4	5 47 13·62	68 13 17·99

Parthenope.

Greenwich M.S. Time.	R.A.	N.P.D.
h m s	h m s	° ′ ″
1855, Dec. 12　9 27 47·6	2 51 49·88	80 21 43·13
15　9 14 40·5	2 50 30·36	80 19 6·55
18　9 1 49·6	2 49 26·99	80 15 16·37
19　8 57 35·1	2 49 8·41	80 13 36·01

Proserpine.

Greenwich M.S. Time.	R.A.	N.P.D.
h m s	h m s	° ′ ″
1855, Dec. 6　11 30 47·1	4 31 30·31	65 42 57·74
19　10 27 16·5	4 19 4·49	66 0 43·44

Psyche.

Greenwich M.S. Time.	R.A.	N.P.D.
h m s	h m s	° ′ ″
1855, Dec. 15　10 13 29·8	3 49 29·31	74 42 12·09
18　9 59 49·6	3 47 36·52	74 44 32·37
21　9 46 19·6	3 45 55·98	74 46 8·70

The north polar distances are corrected for refraction, but not for parallax.

In one or two cases the identity of the object observed for the planet is not quite certain.

Observations of the Solar Spots in the Year 1855.　By M. Schwabe.

	Number of Groups.	Days of no Spots.	Days of Observation.
January	4	5	21
February	3	1	20
March	6	0	20
April	4	12	29
May	5	15	31
June	2	12	28
July	2	27	30
August	1	24	30
September	1	25	27
October	5	6	30
November	3	9	17
December	2	10	21

The above catalogue indicates 313 days of observation and
)served groups of spots, which I could perceive with the
)t Fraunhofer and a magnifying power of 40; with the
)t instrument and a magnifying power of 42, and with·the
; instrument and a magnifying power of 64, I remarked 41
onal small groups, so that, if I retained my earlier system, I
1 have registered 79 groups. On 146 days the sun, when
d with the $2\frac{1}{2}$-foot refractor, appeared free of spots. The
were always small ; their diameter never exceeded 17″;
ι few exhibited any appearance of a penumbra around them.
; were very few penumbræ without a nucleus. On the other
the whole surface of the sun, especially from October, ap-
1 to be diversified with furrows and pores, which gave it a
e aspect.

On several cloudy days, when I could observe the sun with
ιost transparent screen-glass, or even without a glass at all,
ght of the sun's border appeared so remarkably faint, that
:ffect can only be satisfactorily accounted for by a light-
)ling solar atmosphere.

No faculæ were visible on the sun's disk.

)essau, December 31, 1855."

'. Schwabe has prosecuted observations of the solar spots
ut interruption since the year 1826, and has found that the
er of groups is subject to a periodic recurrence. The fol·
g table exhibits the results of his observations for each
;ding year :—

Year.	Days of Observation.	Days of no Spots.	New Groups.
1826	277	22	118
1827	273	2	161
1828	282	0	225
1829	244	0	199
1830	217	1	190
1831	239	3 ·	149
1832	270	49	84
1833	247	139	33
· 1834	273	120	51
1835	244	18	173
1836	200	0	272
1837	168	0	333
1838	202	0	282
1839	205	0	162
1840	263	3	152
1841	283	15	102
1842	307	64	68
1843	312	149	34
1844	321	111	52

Year.	Days of Observation.	Days of no Spots.	New Groups.
1845	332	29	114
1846	314	1	157
1847	276	0	257
1848	278	0	330
1849	285	0	238
1850	308	2	186
1851	308	0	151
1852	337	2	125
1853	299	3	91
1854	334	65	67
1855	313	146	79

In 1852, Dr. Wolf, of Berne, by an examination of old documents, obtained a confirmation of the period deduced by M. Schwabe. He also remarked that the period of the mean annual value of the diurnal variation of the magnetic needle in declination coincides with the period of the solar spots. Dr. Lamont, who first remarked the periodicity of the magnetic phenomenon, inferred that its variations recurred in intervals of about 10⅓ years. Dr. Wolf, however, found that a period of 11·11 years would more satisfactorily represent the observations.

The following synopsis of the magnetic variations from 1835 to 1850, both years included, is extracted from a paper by Dr. Lamont on the subject. The results in the first column have been deduced from the *Göttingen Observations;* those in the second have been established by Dr. Lamont himself at the Munich Observatory:*—

	Mean Diurnal Variation in Declination of the Magnetic Needle.		Mean Diurnal Variation in Declination of the Magnetic Needle.
1835	9·57	1841	7·82
1836	12·34	1842	7·08
1837	12·27	1843	7·15
1838	12·74	1844	6·61
1839	11·03	1845	8·13
1840	9·91	1846	8·81
1841	8·70	1847	9·55
		1848	11·15
		1849	10·64
		1850	10·44

Dr. Lamont has also shown that the observations of Colonel Beaufoy from 1813 to 1820, and the earlier observations of Gilpin and Cassini, indicate the same period of variation.

* Poggendorf's *Annalen,* vol. lxxxiv. p. 572.

Occultation of η Virginis, Mag. 3½, *observed by Capt. Shadwell, R.N., January 26th,* 1856.

		h	m	s	
Immersion	... ...	17	56	32·1	Portsmouth
Emersion	... ...	19	10	59·7	Mean Time.

Observations very satisfactory.

R. N. College, Portsmouth.

Observations of Venus near her Inferior Conjunction.
By Lieut. Brodie.

" I took much interest in closely watching *Venus* for some little time preceding its inferior conjunction, and have several sketches in the Observatory book of its relative appearances. I was unfortunately away from home on the day of the inferior conjunction, but my last observation was on October 4th, four days after the conjunction, when the following entry occurs in the Observatory book : —

" Observed *Venus*, which has passed its inferior conjunction four days ; it is very finely crescented, indeed, and its definition very sharp and clear, the horns extending fully a semidiameter of the planet.

		°	′
The Distance of the Planet from the Sun at the time of this Observation was		9	45
Distance at Time of Conjunction*		8	15
Δ		·1	30

At the close of the meeting the President made some remarks on the increasing importance of the subject of variable stars and the desirableness of multiplying observations of those interesting objects. This was a branch of astronomy which was more especially adapted to the amateur, since a good telescope of moderate power was all that was required for the purpose of observation. M. Argelander, who had distinguished himself by his researches on the periods of variable stars, had employed a method of investigation which was not only very effective in detecting nice gradations of light, but also possessed the advantage of being easily practised by any person who was desirous of prosecuting obser-

* Mr. Brodie, from a hasty reading of the statement attributed to Mr. Airy, at page 232, vol. xv. of the *Monthly Notices*, was led to suppose that the words there used had reference to *daylight* observations of *Venus;* and he mentions that it was for the purpose of testing the remark of the Astronomer Royal, as thus interpreted by him, that he was induced to observe the planet so near the time of conjunction. It will be seen that the words of Mr. Airy plainly refer to the observations of *Venus when the sun is beneath the horizon,* the only time when there exists any probability of discovering a faint crepuscular light in the planet indicative of an atmosphere, the sole object for which the observation of the planet near its inferior conjunction was originally suggested.—EDITOR.

B

vations of this kind. It consisted in selecting a few small stars in the immediate vicinity of the variable object, and employing them as standards of comparison for the purpose of ascertaining the quantity of light which the star emitted while in the course of passing through its successive stages of brightness. It is manifest that the results obtained by this method would be independent of the state of the atmosphere, since any fluctuation arising from this cause would equally affect both the variable star and the star of comparison. It would be, of course, indispensable to its successful application that the stars selected for comparison should be of different magnitudes, and that their light be found by attentive observation to be sensibly steady.

The Hypothesis of Otto Struve respecting the Gradual Increase of Saturn's Ring, discussed with reference to the Manuscripts of Huyghens and the Accuracy of modern Observations. By Professor Kaiser.*

In this memoir Professor Kaiser examines the grounds of the hypothesis recently advanced by M. Otto Struve relative to the existence of a continual variation of *Saturn's* rings. According to that hypothesis, while the diameter of the planet and the exterior diameter of the outer bright ring have constantly preserved the same values, the interior diameter of the inner bright ring has been constantly diminishing ; consequently, the system of bright rings is gradually increasing in breadth, and thereby approaching nearer and nearer to the body of the planet.

The author, after some preliminary remarks, gives an exposition of the numerical results upon which M. Otto Struve founded his hypothesis. Assuming that the exterior diameter of the outer ring has always exceeded the diameter of the planet by $22''\cdot20$, M. Struve found for the breadth of the rings, the breadth of the dark space included between them and the planet, and the ratio of the latter of these two quantities to the former, the results contained in the following table:—

Observer.	Year.	Breadth of Rings.	Breadth of Dark Space.	Breadth of Dark Space. Breadth of Rings.
Huyghens	1657	4·6	6·5	1·41
Huyghens and Cassini	1695	5·1	6·0	1·18
Bradley	1719	5·7	5·4	0·95
Herschel	1799	5·98	5·12	0·86
W. Struve	1826	6·74	4·36	0·64
Encke and Galle	1838	7·06	4·04	0·57
O. Struve	1851	7·43	3·67	0·49

* De Stelling van Otto Struve omtrent het breeder worden van den Ring van Saturnus getoetst aan de handschriften van Huijgens en de naaukeurigheid der latere waarnemingen door F. Kaiser. (Verslagen en Mededeelingen der Koninglijke Akademie van Wetenschappen. Deerde Deel, Amsterdam, 1855.)

The author remarks that of the seven determinations contained in the preceding table there is not one which does not tend to support the hypothesis of M. Otto Struve, that during the last two centuries the ring has been continually increasing in breadth. He adds, however, that if the probable errors of these determinations should exceed in magnitude the variations which they are supposed to undergo, any conclusion deduced from them must be considered as doubtful. It is true that the previous researches of M. Otto Struve afford no reason to suppose that he would have allowed his judgment to be warped by any equivocal considerations; but still it cannot be denied that a preconceived idea may lead the most candid inquirer astray in his reasoning. Moreover, the proneness to exaggerate the accuracy of observations is so common that M. Otto Struve may not impossibly have been led to commit a similar mistake in the course of his investigation.

The author accordingly proceeds to examine the method by which M. Struve arrived at the results contained in the foregoing table. He remarks that hitherto no astronomer had formally called in question the hypothesis of M. Struve, but that several early drawings of the planet had been cited, which, in so far as they might be considered trustworthy, appeared in some instances to confirm the truth of that hypothesis. He is of opinion, however, that these drawings were in all probability founded on the descriptions and drawings given by Huyghens, who devoted more attention to the appearance of the planet than any of his contemporaries, and that, consequently, they cannot be considered as furnishing any independent confirmation of the measures of the Dutch astronomer. Perhaps there may not seem sufficient grounds for the opinion here expressed by Professor Kaiser. There can hardly be any question, however, with respect to the inadequacy of those early sketches in deciding a question of such delicacy as that raised by the researches of M. Otto Struve.

Alluding to the wishes expressed by the Council of this Society that the object-glasses of Huyghens, in the possession of the Royal Society, should be remounted for the purpose of ascertaining the exact influence of irradiation in observations with them, the author has the following remarks:—

"Although I quite agree in the desirableness of ascertaining with greater precision the meaning which ought to be attached to the words of Huyghens, still I cannot refrain from expressing my surprise at the way in which the Royal Astronomical Society of London have proposed to effect this object. The object-glasses of Huyghens, which are preserved by the Royal Society of London, have focal distances of 210, 170, and 120 feet. These glasses were not executed by Christian Huyghens, as has been erroneously supposed in foreign countries, but by his brother Constantine Huyghens. They are all much larger than the largest of the glasses with which Christian ever observed the planet *Saturn*, and more especially they are very large compared with the glasses with which he executed the observations which have formed the

groundwork of M. Struve's theory. Whenever, then, the Saturnian
system is observed with the glasses in the possession of the Royal
Society, it ought certainly to exhibit a different appearance from
that indicated by the descriptions and drawings of Huyghens, even
although it had not in reality undergone any change, and therefore
are such observations incapable in the slightest degree of deciding
the question with respect to the variability or constancy of *Saturn.*

It is plain from the foregoing passage that the author labours
uuder a totally erroneous impression with respect to the object
which was proposed to be effected by the remounting of one of the
Huyghenian object-glasses, in the possession of the Royal Society.
It was, we need scarely mention, for the purpose of ascertaining
the effect of irradiation, not upon Huyghens' observations of
Saturn and his rings, but upon those of Bradley, which were
executed with one of Huyghens' telescopes belonging to the Royal
Society, that the idea of remounting the object-glass employed in
those observations was at one time entertained in this country.[*]

Huyghens, in all his works, has only alluded twice to the
dimensions of *Saturn's* rings, and on both occasions in very brief
terms. In his *Systema Saturnium*, published in 1659, he remarks
that the space between the ring and the body of the planet is
equal to, or even a little greater than, the breadth of the ring
("latitudinem spatii inter annulum globumque *Saturni* interjecti
æquare ipsius annuli latitudinem vel excedere etiam)." His next
allusion to the subject is contained in his *Cosmotheoras*, which
was first published in 1698. On this occasion he asserts that the
space between the ring and the planet is exactly equal to the
breadth of the ring ("vacuum spatium inter utrumque inter-
jectum eandem quam annulus latitudinem habebit").

The mode by which M. Otto Struve determined the ratio of
the interval between the ring and the planet to the breadth of the
ring in both these instances is worthy of remark. According to
his original assumption, the distance from the edge of the planet's
disk to the exterior edge of the ring amounted constantly to $11''\cdot1$.
If the dark space between the planet and the ring *appeared* exactly

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[*] The expression of the author, " in order to ascertain what aspect *Saturn*, in
his present condition, would have exhibited to Huyghens," (" obdat men, omtrent
het voorkomen waaronder *Saturnus* in zijn' tegenwoordigen toestand door
Huijgens zoude zijn waargenomen, zoude worden ingelicht ") does not convey
the exact meaning of the words used in the Annual Report of the Council of this
Society, which were these :—" It appears, however, that to give these observations
their full value, it will be desirable that observations on *Saturn* be repeated with
Huyghens' object-glasses, still preserved, it is understood, in the care of the
Royal Society " (*Monthly Notices*, vol. xiii. p. 129). The Huyghenian object-glass
which Bradley employed in his observations of *Saturn* had been lent by the Royal
Society to his uncle Pound; it had a focal length of 123 feet. This was the
object-glass which the British Association at one time contemplated re-mounting,
for the purpose of testing Otto Struve's theory, but the idea appears to have been
subsequently abandoned. (See Brit. Assoc. Rep., 1852, p. xxxiii.; Rep. 1853,
p. xxiv.) It may be remarked, that the oversight which the author has com-
mitted in this instance does not affect any of the conclusions at which he has
arrived in the course of his discussion.

equal to the breadth of the ring, it must *in reality* have exceeded
the latter quantity, in consequence of the effect of irradiation, and,
therefore, must be somewhat greater than $5''\cdot55$. Supposing the
irradiation to amount to $0''\cdot5$, M. Struve hence concluded that in
1657, when the breadth of the space between the planet and the
ring was considered by Huyghens somewhat to exceed the breadth
of the ring, the former of these spaces must have measured *very
nearly* $6''\cdot5$, and the latter $4''\cdot6$; and he fixes upon these as the
definitive values for the epoch to which they refer, whence he
derives $1\cdot4$ for the ratio of the two quantities. With respect to
the second epoch, the reasoning of M. Otto Struve is certainly
objectionable. Proceeding upon the assertion of Huyghens, that
the space between the planet and the ring, instead of being greater
than the breadth of the ring, as it was in 1657, was now exactly
equal to the latter, he hence concludes that the breadth of the
dark space must, therefore, be less than $6''\cdot5$. But, he remarks, it
must exceed $5''\cdot5$, on account of irradiation. The mean of these
two quantities is $6''$, which he lays down as the breadth of the
dark space for the second epoch. This leaves $5''\cdot1$ for the breadth
of the ring, whence he obtains $1\cdot18$ for the ratio of the two quan-
tities corresponding to the year 1695.*

The author maintains that the reasoning by which these
results have been deduced cannot stand the test of strict dis-
cussion. With respect to irradiation, he is of opinion that it
depends upon the eye of the observer, that its effects are of a
vague and contradictory character, and that the safest course
generally is to take no account of its influence in astronomical
observations. We extract the following passage from his remarks
upon this subject:—

"Granted, however, that an irradiation exists, in virtue of
which luminous objects appear enlarged when projected upon a
dark ground, still it by no means follows that irradiation must
cause dark objects projected upon a light ground to appear too
small. If we assume that the amount of irradiation is the same,
whatever be the magnitude of the dark object, we should then fall

* It will be seen that in determining the breadth of the dark space for the
first epoch, M. Otto Struve adopted a *definite* value of irradiation; but he took
no account of the remark of Huyghens, that the dark space *somewhat exceeded*
the breadth of the ring. On the other hand, his investigation of the breadth of
the dark space for 1695 is founded upon an *indefinite* value of irradiation and
upon the supposition that the words of Huyghens, "vel excedere etiam," *were
duly taken into account in determining the value of the space for* 1657. The
results for the two epochs are not, therefore, comparable: first, because a similar
view of the influence of irradiation was not taken in both investigations;
secondly, because the principles of ratiocination in the two cases are in con-
tradiction with each other. In fact, if the value of irradiation be assumed equal
to $0''\cdot5$ in both cases, the breadth of the dark space must have been somewhat
greater than $6''\cdot5$ in 1657, and must have been exactly equal to that quantity in
1695, or, in other words, the ratio of the two quantities would have amounted to a
little more than $1\cdot41$ at the former of these epochs, and to exactly $1\cdot41$ at the latter
epoch, instead of $1\cdot41$ and $1\cdot18$ respectively, according to M. Otto Struve.

into the absurd conclusion that very small dark objects are changed by irradiation into objects of a greater degree of brightness than that of the ground upon which they are projected, and no relation between the magnitude of the object and the quantity of irradiation can be assumed so long as we possess absolutely no proof of the existence of such a relation."

The author then proceeds to remark that the observations of Huyghens show that dark objects when viewed by him did not appear diminished by irradiation to the extent of $0''\cdot5$. It appears from his Journal that he repeatedly observed the shadow of one of *Jupiter's* satellites upon the body of the planet. In general, the satellites subtend an angle of $1''$, and the apparent diameter of the shadow in any case must, consequently, be somewhat less than that quantity. Upon this ground the author contends that if the effects of irradiation had amounted to $0''\cdot5$, the shadows of the satellites must have been totally invisible to Huyghens,—a result which is at variance with the records of his Journal. He concludes that the safest course is to reject all consideration of the influence of irradiation, and, assuming with M. Otto Struve that in 1657 the dark space between the planet and the ring appeared to Huyghens to be equal to the breadth of the ring, the ratio of the two quantities would be not $1\cdot41$, but simply $1\cdot0$. He, moreover, maintains, upon the ground of the unsatisfactory nature of M. Otto Struve's reasoning, with respect to the influence of irradiation upon the observations of 1695, that the ratio of the dark space to the breadth of the ring for that epoch must also be considered as equal to unity.

The author next gives an account of certain drawings of *Saturn* and his rings which have been found among the manuscripts of Huyghens, preserved in the University of Leyden. They are merely rough representations which Huyghens sketched out with his pen in one of his observation-books.* The earliest drawing refers to the appearance of the planet on the 27th of December, 1657; the latest to his appearance on the 24th of August, 1693. The total number of drawings amounts to twenty-three. Selecting those that appeared most worthy of confidence, the author carefully examined them for the purpose of ascertaining what light they were calculated to throw upon the subject of discussion. The following are the values of the ratio of the dark space to the breadth of the ring which he obtained for four distinct epochs by actual measurement of the drawings:—

$$\frac{\text{Breadth of Dark Space}}{\text{Breadth of Rings.}}$$

1657, Dec. 27	... ...	0·72
1675, Dec. 8	... ...	0·95
1682, Oct. 16	... ...	1·01
1693, Aug. 24	... ...	1·24

* Professor Kaiser's paper contains copies of three of these drawings, referring severally to the appearance of the planet in the years 1657, 1675, and 1693.

It would appear from these results that during the lifetime of Huyghens the ring, instead of gradually increasing in breadth, was, on the contrary, growing narrower. The author, however, is of opinion that the observations of the Dutch astronomer are incapable of leading to any trustworthy conclusions on the subject.

" A mere comparison of the drawings of Huyghens with the most recent measures cannot," says he, " decide anything respecting the variability or constancy of the rings of *Saturn*. The latest measures of *Saturn's* rings, namely, those which have been executed by Mr. Main at Greenwich, in the beginning of the year 1853, assign 0·82 for the ratio of the dark space to the breadth of the ring. The drawings of Huyghens assign to this ratio all values included between 0·72 and 1·24. We might, therefore, by a comparison of the observations of Huyghens with those of recent times, make out that the ring was gradually becoming broader or narrower, according as we found either hypothesis convenient for our purpose; and we shall, therefore, do best by considering the observations to be totally unsuitable for an investigation such as that undertaken by M. Otto Struve."

After an examination of Bradley's and Herschel's observations of *Saturn* and his rings, the author next considers the modern observations, namely, those of W. Struve, Encke, Galle, Bond, Otto Struve, and Main. On the observations of the latter, which were executed in 1852–3, the author has the following remarks :—

" These measures have now furnished a result which for once is at variance with the hypothesis of Otto Struve. They assign 0·80 for the ratio of the dark space to the breadth of the ring, a value which exceeds the ratio obtained by W. Struve, as much as the ratio of Otto Struve falls below the latter. If, then, the measures of Otto Struve demonstrate that since the year 1826 the ring of *Saturn* has sensibly increased in breadth, the observations of Main, on the other hand, indicate an equal diminution of breadth for the same period. If we assume that for the smaller, and perhaps less‡perfect telescope of Main, the amount of irradiation is much greater than for the telescope of Pulkowa, the measures would be still more unfavourable to the hypothesis of Struve. Admitting that the irradiation in Main's telescope amounted to merely a quarter of a second, we should then obtain 0·95 for the ratio of the dark space to the breadth of the ring, a result agreeing with that which M. Struve deduced from the measures of Bradley, executed 134 years earlier, and also with the ratio indicated by the most complete drawing of *Saturn*, contained in the Journal of Huyghens, which was written 177 years previously. From the discordance between Otto Struve and Main we may form an opinion of the degree of confidence to which the measures and estimations of the seventeenth and eighteenth centuries are entitled."

The author concludes by asserting that there exists no reason

whatever for supposing that the compound ring of *Saturn* is gradually increasing in breadth.*

* Professor Kaiser's paper contains a variety of critical remarks upon the advantages and defects of the heliometer, the wire micrometer, and the double-image micrometer, in measures such as those relating to *Saturn's* rings. He is inclined to prefer the modified form of Airy's double-image micrometer suggested by Valz (*Monthly Notices*, vol. x. p. 160), and, with the view of testing the justness of the conclusion at which he arrived, he has ordered one of such micrometers of Mr. Simms, which he purposes applying to the telescope of the Leyden Observatory.

CONTENTS.

ROYAL ASTRONOMICAL SOCIETY.

THE Annual General Meeting of the Society, MANUEL J. JOHNSON, Esq. President, in the Chair.

Andrew Barclay, Esq., Kilmarnock,
Charles Tennant, Esq., Glen, Peeblesshire, and
Capt. Norman M'Leod, Local Marine Board, Liverpool,

were balloted for and duly elected Fellows of the Society.

Report of the Council to the Thirty-sixth Annual General Meeting of the Society.

The Council can, as has so often happened before, meet the Society with expressions of satisfaction at the general state of astronomy, both at home and abroad. But though science lives, its supporters are removed, one by one, and the great interests of humanity are left to find new promoters. The past year has been saddened to us by the almost sudden death of Mr. Sheepshanks, one of our most valuable colleagues and one of our oldest members.

The Report of the Auditors, subjoined, will show the state of the finances :—

RECEIPTS.

	£	s.	d.
Balance of last year's account	314	11	8
By dividend on £2878 17s. 5d. new 3 per Cents	40	13	3
By ditto on £1650 Consols..	23	2	0
By ditto on £2878 17s. 5d.	40	6	1
On account of arrears of contributions	103	14	0
106 contributions (1855–56)	222	12	0
8 ditto (1856–57)	16	16	0
Carried forward	£761	15	0

	£	s.	d.
Brought forward......................	761	15	0
5 compositions	105	0	0
21 admission fees	44	2	0
18 first year's contributions	33	12	0
Sale of Publications	67	17	0
	£1012	6	0

EXPENDITURE.

	£	s.	d.
Cash paid Mr. Grant...	9	10	0
Ditto	20	0	0
Mrs. Jones (Lee Fund)	4	18	3
J. Rumfitt, bookbinder	3	14	0
Mr. Grant	10	0	0
George Bárclay, printer	144	13	2

Taxes { 1 year's land tax 5 12 6
{ 1 year's property tax 2 18 4

	£	s.	d.
	8	10	10
J. Williams' salary	100	0	0
Ditto commission on collecting £488 13s. 0d..........	24	8	6
Charges on books, and carriage of parcels	2	12	9
Postage of letters and Monthly Notices	13	16	8
Porter's and charwoman's work	25	14	2
Tea, sugar, biscuits, &c. for evening meetings	13	13	0
Coals, candles, &c...	15	6	6
Waiters attending meetings	3	17	0
Sundry disbursements by the Treasurer	21	12	0
Expenditure of Turnor Fund	11	15	7
Balance in the hands of the Treasurer	578	3	7
	£1012	6	0

Assets and present property of the Society :—

	£	s.	d.
Balance in the Treasurer's hands	578	3	7

				£	s.	d.
1 contribution	of 8 years' standing	....	16 16 0			
4 ———	of 7	ditto	58 16 0			
2 ———	of 6	ditto	25 4 0			
1 ———	of 5	ditto	10 10 0			
2 ———	of 4	ditto	16 16 0			
7 ———	of 3	ditto	44 2 0			
8 ———	of 2	ditto	33 12 0			
22 ———	of 1	ditto	46 4 0			
				252	0	0

	£	s.	d.
Due for publications of the Society..............................	1	0	0

£1650 3 per Cent Consols.
£2878 17s. 5d. new 3 per Cent.
Unsold publications of the Society.
Various astronomical instruments, books, prints, &c.

	£	s.	d.
The balance of the Turnor Fund (included in Treasurer's balance above)...	29	14	5

Stock of volumes of the *Memoirs :—*

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	36	VII.	221	XVII.	243
I. Part 2	80	VIII.	207	XVIII.	242
II. Part 1	99	IX.	212	XIX.	259
II. Part 2	65	X.	224	XX.	265
III. Part 1	127	XI.	235	XXI. Part 1 (separate).	302
III. Part 2	143	XII.	243	XXI. Part 2 (separate).	101
IV. Part 1	150	XIII.	259	XXI. (together).	154
IV. Part 2	160	XIV.	445		
V.	172	XV.	252	XXII.	251
VI.	197	XVI.	255	XXIII.	444

Progress and present state of the Society :—

	Compounders.	Annual Contributors.	Non-residents.	Patrones, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1855	151	186	61	6	404	60	464
Since elected	3	17	...	...	20	...	20
Deceased	—3	—3	...	—1	—7	—2	—9
Removals	2	—2	...	...	...	...	...
Resigned	...	—1	...	...	...	...	...
February 1855	153	197	61	5	417	58	475

The instruments belonging to the Society are now distributed as follows :—

> The *Harrison* clock,
> The *Owen* portable circle,
> The *Owen* portable quadruple sextant,
> The *Beaufoy* circle,
> The *Herschelian* 7-foot telescope,
> The *Greig* universal instrument,
> The *Smeaton* equatoreal,
> The *Cavendish* apparatus,
> The *Lee* circle,
> The 7-foot Gregorian telescope (late Mr. Shearman's),
> The Universal quadrant by Abraham Sharp,
> The *Fuller* theodolite,
> The Standard scale,

are now in the apartments of the Society.

The Brass quadrant, said to have been *Lacaille's*,

is in the apartments of the Royal Society.

The remaining instruments are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

> The *Beaufoy* clock,
> The two invariable pendulums, } to the Royal Society.
>
> The Variation transit (late Mr. Shearman's), to Mr. Gravatt.
>
> The other *Beaufoy* clock, to the Rev. J. B. Reade.
>
> The *Wollaston* telescope, to the Rev. T. W. Webb.

The Council have great pleasure in stating that the Society continues to receive from time to time valuable donations of different kinds calculated for the advancement or illustration of our science. Among the recent accessions to the Library of the Society arising from this source may be mentioned a copy of a new edition of the works of Galileo, in twelve volumes, royal octavo, which was prepared for the press by a careful examination of the original manuscripts of the author, in the possession of the Grand Duke of Tuscany. It is understood to be the first complete edition of the works of the illustrious Italian which has hitherto been published. For this precious gift the Society is indebted to the Authorities of the Imperial and Royal Museum of Physics and Natural History of Florence. The Council would also beg to call the attention of the Society to a collection of very beautiful photographs of the moon, which were executed with the great equatoreal of Cambridge, U. S., and which have been recently presented to the Society by Mr. Bond.

Every succeeding year shows the advantage of having a small sum, such as is yielded by the Turnor Fund, to expend in the purchase of books. The additions made to our library from this source are slowly, but surely, rendering it worth the attention of the astronomical antiquary. The Council could, at any time, have afforded an equal annual amount for the purchase of old books, if the wants of the Fellows had demanded such an expenditure. But there exists so little taste for antiquarian investigation, that no such thing was ever proposed. The Turnor Fund may be one more instance of a useful but neglected branch of a subject the cultivation of which is fostered by an endowment.

It will be proposed to the meeting, on the part of the Council, to make such addition to the Bye-laws as will add an ordinary meeting of the Society in July to those already customary. This experiment is a consequence of some discussions the tenor of which it may be desirable to lay before the Society, with a view to bringing into notice the question whether other changes may not be desirable.

It is well known that since the formation of the Society the London season has considerably altered, both its beginning and ending having been advanced in time. The Christmas vacation is practically longer, and the month of July is fully as much within the working political and scientific year as the month of June was thirty years ago. Under these circumstances, it was discussed whether it would not be advisable to discontinue the meeting in January, and to substitute for it a meeting in July. The Council decided to recommend the addition, but not, as yet at least, the counterbalancing subtraction.

Again, the month of February has various disadvantages with reference to the annual meeting and the anniversary dinner which follows. It is very early in the political season, and the weather is seldom favourable. If the anniversary were placed in May, the month in which London is so full, both of residents and visitors, it is very likely that a much better attendance would be procured. The Council have not decided on any alteration except the one of which notice has been given. But they are desirous of having this subject discussed, and hope to learn the opinion of the Fellows at large, when due time for consideration shall have elapsed.

The Medal has been awarded to Mr. Grant, for his *History of Physical Astronomy*, a work which, from its first appearance, has been felt to supply an urgent want, and is now entitled, by the tests which have been applied to it, and by the resulting opinion formed of it, to rank as an astronomical classic. The President will deal with this award in the usual manner.

The twenty-fourth volume of the *Memoirs* has just been published. It contains several communications which cannot fail to attract the attention of all those who take an interest in the progress of our science. The paper by the Astronomer Royal on the determination of the difference of longitude of the Observatories of Greenwich and Brussels by galvanic signals indicates the application of a method which has only been devised within the last few years, for more effectually comparing the recorded times of observations at two distant places on the earth's surface. The satisfactory nature of the results which had been already obtained in America by employing the agency of galvanic electricity in effecting an astronomical connexion between various stations of importance throughout the Union, has been amply borne out by the operations of a similar character detailed in Mr. Airy's paper. The consistency of the individual observations leaves nothing to be desired, and the definitive value of the difference of longitude of the two Observatories may be considered as a faithful exponent of the degree of accuracy attainable in researches of this kind when conducted by the aid of some of the most refined appliances of modern science.

It will be remembered, that in a letter to the Astronomer Royal, which was published in the *Monthly Notices* for November

1854, M. Hansen announced that he had discovered a series of irregularities in the values of the principal equations of the moon's longitude which he was unable to account for by the existing state of the lunar theory, and which led him to suspect that they might be occasioned by a displacement of the moon's centre of gravity relative to its centre of figure. This idea suggested to M. Hansen the investigation of the consequences which would ensue if such a displacement really existed in nature; and an elaborate paper on the subject, which he communicated to the Society, appears in the volume of *Memoirs* for the past year. While the Fellows of the Society will, therefore, now have an opportunity of examining how far the results obtained by M. Hansen tend to confirm the hypothesis upon which they are based, they assuredly cannot fail to admire the consummate mathematical ability with which the author has treated so intricate a question.

The theory of atmospheric refraction is so intimately associated with the accuracy of astronomical determinations, and still leaves so much to be desired in the way of further improvement, that each successive attempt to develope more fully its principles by the aid of sound physical considerations cannot fail to be hailed with general satisfaction. It is under a strong conviction of the promptitude with which the Fellows of the Society will acquiesce in the justness of this remark, that the Council would beg to call their attention to a valuable paper by Sir John Lubbock, embodying his researches on this important subject, which is inserted in the volume of *Memoirs* just published.

The same volume also contains an important paper by Mr. Main on the determination of the constants of nutation and aberration, including a simultaneous investigation of the parallax of γ *Draconis*. The materials which formed the groundwork of Mr. Main's researches on this occasion consisted of an extensive series of observations of γ *Draconis*, executed with the twenty-five-foot zenith tube of the Royal Observatory. The utmost precaution was employed in examining and guarding against every circumstance which might be conceived to exercise a disturbing influence on the observations, and the whole investigation has been rigorously conducted according to the most refined principles of modern computation. The resulting value of the constant of nutation exhibits a satisfactory agreement with the various determinations of that element which have been deduced from the most trustworthy researches of recent times, and must be considered as forming a valuable contribution to our knowledge on the subject. The resulting parallax of the star is negative, and this anomalous circumstance, as well as the smallness of the deduced constant of aberration, affords reason to suspect that the observations have been affected in a slight degree by some unknown disturbing cause of a variable nature, and, in all probability, of a short period. From the excellence of the observations, the anomaly thus presented is interesting and important, and although it may be difficult to discover its origin and eliminate its effects, still it cannot

be doubted that it is regulated by some fixed law, the complete elucidation of which on some future occasion will be productive of advantage to astronomy, as has happened in numerous other instances of a similar kind recorded in the annals of our science.

With respect to the *Monthly Notices*, it is only necessary to state, that they still continue to be conducted upon the plan which has been for some time pursued, and which has been found to give general satisfaction to the Fellows of the Society. Every astronomical phenomenon of any interest, every useful computation, however modest may be its pretensions, finds a ready vehicle of publication in the pages of this journal. It must be obvious to every Fellow of the Society, that the usefulness of the*Monthly Notices* will be most effectually promoted, not by confining its pages mainly to any one branch of our science, but by so adjusting its subject-matter as to enlist in its behalf the services and excite the unceasing attention of every class of astronomers throughout the country. The attention of the Council continues to be seriously directed to every circumstance which may appear calculated to facilitate the attainment of so desirable an object, so far as it can be accomplished without any undue expenditure of the funds of the Society. We need scarcely say that it always affords us great pleasure to receive communications from our foreign friends, either directly or through any Fellow of the Society. Although it has been found most convenient to adhere to the English language in the editing of the *Monthly Notices*, the Council would not the less cordially invite contributions from other countries, since they will always endeavour, consistently with a due regard to the general character of this journal, to give currency to any results which may appear to them to be calculated to promote the advancement of our science, whatever language may have been employed as the vehicle for communicating such results to the Society.

The Council have to regret the loss by death of our Associates, M. Augustus Ludovick Busch, M. Charles Frederick Gauss, and General Ferdinand Visconti; and also of the following Fellows :— Lieutenant George Beaufoy ; Bryan Donkin, Esq. ; Sir Robert Harry Inglis ; Henry Lawson, Esq. ; Joseph Parkinson, Esq. ; William Devonshire Saul, Esq. ; Rev. Richard Sheepshanks.

The Council are desirous of recording some particulars of the career of every deceased Fellow: but it is obvious that without the assistance of the immediate representatives this object cannot be attained. It does not always happen that information of the decease reaches the Council in time for the next Annual Report. Application is always made for the requisite particulars as soon as due respect to the feelings of surviving relatives will permit: but this application is not always attended to.

AUGUSTUS LUDOVICK BUSCH was born at Dantzic on the 7th of September, 1804. His parents were in wealthy circumstances

at the time of his birth, but the bombardment of Dantzic by the French, in 1813, reduced them to a condition of poverty.

After acquring a knowledge of the ordinary branches of education, Busch entered the Royal School of Arts in Dantzic. The Director of this institution, John Adam Breysig, possessed a peculiar aptitude for awakening in his pupils a love of geometrical drawing and also of geometry itself. Under his tuition young Busch made considerable progress in several useful applications of geometry. Subsequently he associated himself with an architect named Pape, whom he assisted with his drawings and measurements. Not being inclined to adopt the profession of an architect, he turned his attention to the study of pure mathematics, which he cultivated unner the guidance of Förstmann, Professor of Mathematics in the Gymnasium of Dantzic, who instructed him privately for several years free of charge.

In the year 1827, having proceeded to Königsberg, he was appointed private tutor to the children of the poet Freihern, of Eichendorff, who was then Catholic Consistorial Councillor of that city. While occupying this situation he enjoyed the advantage of attending the lectures of Bessel and the other professors of the University.

In the year 1831 he was appointed assistant to Bessel, who was then Director of the Observatory of Königsberg. The labours of Busch in this situation are well known to all those who are in the habit of perusing the *Königsberg Observations* and the *Astronomische Nachrichten*. In the year 1833 Busch undertook the reduction of Bradley's observations with the zenith sector. The results were published in 1838 under the title of *Reduction of the Observations made by Bradley at Kew and Wanstead to determine the Quantities of Aberration and Nutation*. In 1849 he was appointed to the Directorship of the Königsberg Observatory, which had become vacant by the death of Bessel. He died of cholera on the 30th of September, 1855.

CHARLES FREDERICK GAUSS was born at Brunswick on the 30th of April, 1777. His father, who was a bricklayer, intended that his son should adopt the same occupation. Accordingly, in the year 1784, young Gauss was sent to the public school of Bütner, in Brunswick, for the purpose of being instructed in the ordinary elements of education. During his attendance at this school, his extraordinary intelligence attracted the notice and procured for him the friendship of Bartels, subsequently Professor of Mathematics in the University of Dorpat, and father-in-law of our celebrated Associate, M. Struve, Director of the Imperial Observatory of Pulkowa. Bartels having kindly represented the merits of young Gauss to Charles William, Duke of Brunswick, he was sent, in the year 1792, to the Collegium Carolinum, very much against the will of his father, who perceived that his own intentions with respect to the future calling of his son would thereby be completely frustrated. In 1794 he entered the University of Göttin-

gen, not yet quite decided whether he should devote his life to the pursuit of mathematics or philology. During his residence here he made several of his greatest discoveries in analysis, which induced him to make the cultivation of mathematical science the main object of his life.

Having completed his studies, he returned to Brunswick, and, in 1798, he repaired to Helmstadt for the purpose of availing him-self of the library of that place, having been then engaged in pre-paring for publication his celebrated work, *Disquisitiones Arith-meticæ.* Shortly after his arrival he was introduced to Pfaff, but he was merely in company with him for an hour or two. Upon his return to Helmstadt, however, in the following year, with the same object in view, he had the opportunity of renewing his ac-quaintance with Pfaff, which soon ripened into a very intimate friendship. In the course of their evening walks they were in the habit of exchanging their thoughts on mathematical subjects, on which occasions it may be presumed that Gauss communicated quite as much as he received. It has been considered necessary to state these facts in consequence of an erroneous impression which has very extensively prevailed, even in Germany, that Gauss studied mathematics at Helmstadt under the tuition of Pfaff. The *Disquisitiones Arithmeticæ* was .published at Brunswick in 1801, under the auspices of the Duke of Brunswick. It immediately stamped its author as one of the most profound and original mathematicians of the age.

The discovery of the planet *Ceres* by Piazzi on the first day of the present century had the effect of introducing Gauss to the world as a theoretical astronomer of the very highest order. The Italian astronomer not having communicated a sufficient number of his observations of the planet previous to its passing into the rays of the sun, which happened soon after its discovery, there existed no means of ascertaining the form or position of the orbit in which it revolved; and the consequence was, that upon its emerging again from the solar rays in the autumn of the same year, astronomers were totally unacquainted with the precise region of the heavens in which they ought to search for it. Piazzi having at length published his early observations of the planet, Gauss, by a method of his own invention, determined the elements of its orbit, and calculated an ephemeris of its motion, by means of which De Zach succeeded in re-discovering the planet on the 31st of December, exactly after the lapse of a year from the date of its original discovery by Piazzi. The discovery of three other small planets, which soon followed that of *Ceres*, supplied Gauss with so many occasions for improving his solution of the problem for determining the orbit of a planet from a definite number of obser-vations, and suggested to his inventive mind a variety of beautiful contrivances for computing the movement of a body revolving in a conic section in accordance with Kepler's laws. These results were finally embodied in his *Theoria Motus Corporum Cœlestium in Conicis Sectionibus Solem Ambientium*, which was published at

Hamburgh in 1809. In this celebrated work the author gives a complete system of formulæ and processes for computing the movement of a body revolving in a conic section, and then explains a general method for determining the orbit of a planet or comet from three observed positions of the body. The work concludes with an exposition of the method of least squares, which the author appears to have invented independently of, and even prior to, Legendre, although the latter was the first who communicated it to the world.

The *Theoria Motus* will always be classed among those great works, the appearance of which forms an epoch in the history of the science to which they refer. The processes detailed in it are no less remarkable for originality and completeness than for the concise and elegant form in which the author has exhibited them. Indeed, it may be considered as the text-book from which have been chiefly derived those powerful and refined methods of investigation by which the German astronomy of the present century is more especially characterised.

It is a curious fact that the date of the preface to this immortal work is exactly two centuries later than the date of Kepler's equally renowned work *De Stella Martis.* The former is dated March 28, 1809; the latter is dated March 28, 1609.

The other astronomical researches of Gauss are chiefly contained in De Zach's *Monatliche Correspondenz,* the *Transactions of the Royal Society of Göttingen,* and the *Astronomische Nachrichten.* Although not of equal importance with those expounded in the *Theoria Motus,* they all bear the impress of original genius.

In 1807 Gauss was appointed Professor of Mathematics at Göttingen, where he continued to reside during the remainder of his life. Latterly he devoted considerable attention to the subject of terrestrial magnetism, and in concert with Professor Weber made some very important improvements in that branch of science. He died on the 23d of February, 1855. His remains were accompanied to the grave by a vast multitude of persons, including the entire corps of the University of which he was so distinguished an ornament.

Gauss was one of the leading mathematicians of the age, and was the last of the powerful school which is headed by Lagrange; but he lived to an age which made him the survivor of many who must be said to belong to a later epoch. His researches are of the most abstruse character, and turn much on the theory of number and its applications. The *Disquisitiones Arithmeticæ* is one of the standard works of the century. But though the character of his subjects tempts few readers—though his own severe brevity renders these subjects even more difficult than they need be—yet the young reader of Euclid may be brought into contact with Gauss, so as to understand the tone of his genius in a manner which would be utterly impossible in the case of Newton, or Lagrange, or Euler.

It had always been supposed that Euclid had attained the

boundary of what is possible in geometrical construction, with the allowance of constructive means to which he limits himself by the three first postulates. Two thousand years had past without any construction being achieved of which a geometer would have supposed Euclid or Archimedes incapable, had the attention been turned that way. But when Gauss, by the highest algebra applied to numerical considerations, showed that a regular polygon of 17 sides (or of any number which is prime, and also one more than a power of 2) can be inscribed in a circle under Euclid's restriction as to means, he made the first advance upon Euclid, and established the connexion of trains of thought so widely different in character, in subject-matter, and in difficulty, that his theorem is of a most useful application. It is the most remarkable standing proof that every part of mathematics must be looked into for the progress of every other; and we have no doubt that this theorem has very much encouraged research into the hidden points of relation between the different branches of pure science.

It was reserved to Gauss to open that extension of plane geometry which consists in transferring the field of reasoning from a plane to any surface whatever. Every surface has its shortest line, as a plane has its straight line; and a triangle drawn upon a surface, bounded by shortest lines, such as the common spherical triangle on the surface of a sphere, has close analogies with the rectilinear triangle in a plane. Gauss showed how the sum of the three angles of such a triangle is connected with the constitution of the area inclosed; thus extending to all surfaces the well-known theorem which Roy and Legendre applied in geodetical calculation. The time may come when the advance of mathematical reasoning shall convert plane geometry into a geometry of all surfaces, in such manner that any theorem which is established on one surface shall immediately be read off on every other. Should this time ever arrive, it will be remembered that Gauss first opened the career, and suggested the possibility of the extension, by giving some of the principal theorems.

The Council have not till now been able to procure any account of the life of the late General FERDINAND VISCONTI. He was born in Palermo in the year 1772, and educated in the Scuole Pié of that city, whence he was placed among the cadets for the army. Whilst still at the Military College he was arrested, and confined, without any distinct charge against him, in the dungeons of the island of Pantellaria, where he was placed in a small cell with three other political victims, who soon died in misery. Although liberated in 1801, he was obliged to expatriate himself and flee to Milan, where he entered the corps of Engineers, and rapidly distinguished himself as an expert geographical astronomer.

Visconti was charged by the Emperor Napoleon with the construction of a new large military and administrative map of Lombardy. The materials placed at his disposal were so imperfect that he was compelled to begin by fixing for himself the

latitudes and longitudes of numerous principal places and points of the triangulation. In 1810 he accompanied General Danthouard into the Tyrol, to fix the boundaries between Bavaria and the new kingdom of Italy. In 1814 he was permitted to return to Naples, where he was soon placed at the head of the Bureau Topographique, which he speedily placed on a sound and serviceable scientific footing; and on the return of Ferdinand IV. from Sicily, was confirmed in his office. In the Carbonari commotion of 1820, he was nominated by the hereditary Prince as a Member of the Provisional Junta of the Constitutional Government; yet, when King Ferdinand arrived with the Austrians, our excellent associate was dismissed from all his employments. Between 1822 and 1826 he was a wanderer; but in the latter year the hereditary Prince restored and promoted him.

The principal works of General Visconti were: the map of Italy, already alluded to, and a large topographical map of Naples and its environs—works evincing a rare union of accuracy and artistic talent. Between the years 1817 and 1820 he was a zealous co-operator with Rear-Admiral Smyth in the Survey of the Adriatic Sea and its shores.

Lieutenant GEORGE BEAUFOY was a son of the late Colonel Mark Beaufoy, of Bushey Heath, so well known as a practical astronomer and for his nautical tables founded on a long series of hydraulic experiments conducted by himself. Bent upon maritime life, Mr. Beaufoy was placed in the Royal Navy in the summer of 1810, on board the Elizabeth of 74 guns, then commanded by his father's friend, the Hon. Capt. Henry Curzon. Subsequently he served in various ships on the West-Indian and Mediterranean stations; and, after witnessing the fall of Genoa in 1814, he went to the East Indies in the Iphigenia, Capt. Andrew King, to whom he proved useful as being a scientific navigator. Still it was not till the year 1821 that Mr. Beaufoy obtained a lieutenant's commission, when serving on board the Forte frigate on the Halifax station, whence he was paid off in 1824. Having remained some time on half-pay, he joined the Samarang in 1828, and from that date to 1845 served on the North American, West Indian, African, and Home stations, till disappointment at non-preferment and deteriorated health drove him again to half-pay, and he thenceforth remained unemployed till his death.

It is known to the members of the present meeting that Colonel Beaufoy—who was one of our first Fellows—bequeathed the valuable instruments of his excellent private observatory to this Society; and for the marked attention with which Lieut. Beaufoy conveyed the bequest, he was placed upon your honorary list.

BRYAN DONKIN was born at Sandoe, in Northumberland, March 22, 1768. His celebrity in the profession of an engineer, in which he passed the greater part of his long life, is not within our province to describe in detail. He prepared the heavy parts

of the zenith micrometer, which was made by Troughton for the Royal Observatory. His practical completion of the machine for making paper, and · his conquest over the difficulty which the original inventor left to him,· would alone place his name high in the list of useful inventors. His improvements in the printing machinery maintained his reputation ; while his well-known method, to which his name has been attached, for preserving meat and vegetables for long voyages at sea, has added to the safety of many and to the comfort of multitudes.

Mr. Donkin is best known to astronomers by his dividing engine and by his level.

The dividing engine consisted of an application of Maudslay's method of compensating the erroneous length of a screw by a bent lever and straight bar. This method was applied to the intermediate errors by the use of a curve obtained experimentally by continual bisections.*

Mr. Donkin's level may be understood by conceiving a slender spring fixed at its base, to stand upright, and to have a little weight at the top. If the weight be very small, the spring will stand majestically. If large, the spring will bend down. If of a certain magnitude, the spring will stand upright; but a very little force, or a very little alteration of adjustment, will make it incline much. In this state, if the base-piece be inclined a little, then the spring will incline very much, so that every tilt of the base-piece will be enormously exaggerated in the inclination of the spring.

In applying this contrivance to the transit instrument, Mr. Donkin constructed a bar resting with forks upon the pivots of the transit, and fixed the spring on the top of this bar. Then, with a small microscope fixed in the box which shrouded the spring, he observed on a further magnified scale the inclination of the spring.†

Mr. Donkin's pursuit of practical astronomy was with him a mere recreation, and extended little beyond the regulation of his clock by the transit instrument, the occasional observation of an eclipse, or an occultation of a star by the moon, or the determination of a latitude or longitude with a sextant or reflecting circle. His instruments were of the very best, for he would never work with an indifferent tool, if it were possible to procure a good one. His only fixed instrument was a transit, which, with an excellent regulator, stood in a neat little observatory. He was a good judge of a telescope, and possessed two of the best ever made by the elder Tulley. He was expert in the use of the micrometer, and knew well how to handle and make the most of such instruments as are especially intended for the scientific traveller.

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* A detailed description of this contrivance will be found in Holtzapffel's *Turning and Mechanical Manipulation*, vol. ii. p. 651.

† A spring in this position had been used dynamically before as a pendulum vibrating slowly and admitting of being adjusted to correspond to the vibrations of another pendulum, in which it would be shown whether a pendulum-stand was shaken by its pendulum. It was called a "Noddy." But the statical use is purely Donkin's.

Mr. Donkin died February 27, 1855. He had long been a Member of the Society, and was frequently in the Council. As an adviser in the matters to which he had especially attended, and not in them alone, his aid was of the highest value; and his moral worth and kindness of heart and of manner added to his weight and influence. For some years previous to his death he was unable to attend our meetings; but as long as it was possible, and up to a very great age, he was active wherever his services were required.

Sir Robert Harry Inglis died at his residence in Bedford Square, on the 5th of May, 1855, in his seventieth year, deeply regretted by a large circle of friends of all persuasions and pursuits: for though strictly consistent in his own views during a long political career, his good sense, moderation, and invariably amiable demeanour, had endeared him to all. With his acknowledged assiduity in his parliamentary duties we have nothing to do here; but as a firm and early friend of this Society, and for his active zeal in promoting the efficiency of the Radcliffe Observatory at Oxford, he is well worthy of our respectful recollection. Sir Robert was, moreover, an elegant scholar, and well versed both in classical and English literature; hence he had long been a distinguished member of our leading societies.

Henry Lawson was born at Greenwich on the 23d of March, 1774. He was second son of the Very Rev. Johnson Lawson, Dean of Battle. His mother was Elizabeth, eldest daughter of Henry Wright, Esq., of Bath, a gentleman of considerable standing, being twice mayor of that city; she was thrice married. Her third husband was Edward Nairne, the eminent optician, of Cornhill, London, who died in 1806. It appears that Henry and his brother were pupils of the celebrated Dr. Burney, of Greenwich. They quitted school at an early period, and were apprenticed to Mr. Nairne. From some cause neither of them followed the business of an optician; indeed, Henry never entered into any trade or profession. No doubt the connexion with Mr. Nairne caused Henry's attention to be directed at an early age to those scientific subjects to which in after-life he devoted so much of his time. During the mother's life (she died in 1823, and was buried at Greenwich, having survived her third husband about seventeen years) Henry never kept house, living at Chelsea with Mr. and Mrs. Nairne, yet having lodgings in London for convenience. Wherever he was located he always had a room which he converted into a workshop, and in which he spent a large portion of his time.

Mr. Lawson was descended from Katharine Parr; Miss Agnes Strickland, in the life of that queen in the *Queens of England,* says that she has presumptive evidence that he derives his descent from the daughter of Katharine Parr. Some relics of Katharine Parr's personal property descended to Henry Lawson as heir-looms.

These relics Mr. Lawson bequeathed to Miss Strickland, who is also a descendant of Katharine Parr. They consisted of " The Picture of Katharine Parr; " — the napkin which had descended to the Queen from the first Queen of Henry VIII.;—the Arms of England, engraved on copper, which had occupied the centre of a large dish, and belonged to Henry VIII.;— a large gold ring containing Queen Katharine's hair;—an oil picture of Henry VIII.;—a miniature picture of his son King Edward VI.;—and a number of papers on the subject.

Henry Lawson, at the close of 1823, married Amelia, only daughter of the Rev. Thomas Jennings, vicar of St. Peter's, Hereford. From this time he resided at Hereford till the death of a relative (Miss Westwood), who left him a considerable fortune. In 1841 he moved to No. 7 Lansdowne Crescent, Bath, where he resided until his death, which occurred but a few weeks after that of his wife's. In 1820, Mr. Dollond supplied him with a 2½-foot telescope; in 1826, with a remarkably fine 5-foot telescope; in 1834, with his celebrated 11-foot telescope; and in 1841, with the atmospheric recorder. These and numerous other instruments were removed to Bath, where he had converted the roof of his house into an observatory. Indeed, from the time of his marriage he spared no expense in the construction of his astronomical and meteorological observatory. After his removal to Bath he had for some time weekly conversational parties, to whom the large telescope was naturally an object of much interest. Both at Hereford and Bath he was accustomed to record such astronomical, meteorological, and other observations, including the accounts of all earthquakes; and, in short, anything curious in nature of which he thought a record would be useful, in manuscript books which he had for the purpose. It is to be regretted that all these manuscripts were disposed of at the sale of his house and furniture.

Mr. Lawson was elected a Fellow of the Royal Astronomical Society in 1833, a Fellow of the Royal Society in 1840, and a Member of the British Meteorological Society in 1850. To each of these Societies he has bequeathed the sum of 200*l.*, free of legacy duty. In 1796 a number of young men united to form a philosophical body called the Askesian Society (an account of which will be found in the Appendix to Howard's *Barometrographia* and also in the *Life of William Allen, F.R.S.*), the objects of which were to elucidate, by experiment, either facts generally understood, or to examine and repeat any novel discovery. The meetings were held twice a-month during the winter recess, first at Mr. Allen's and afterwards at Dr. Babington's. At these meetings each member in turn was expected to produce a paper upon some subject of scientific inquiry, and many of these papers were afterwards published in *Tilloch's Philosophical Magazine.* Amongst the members were,—William Allen, William Phillips, Luke Howard, Joseph Fox, Henry Lawson, Arthur Arch, W. H. Pepys, Samuel Woods (the President), Astley Cooper, Dr. Babington, Joseph Ball, Richard Phillips, A. Tilloch,

. and Joseph Woods, jun. This Society, limited at first to fifteen, and afterwards to twenty members, fully answered its original objects, and continued its labours until superseded by the formation of the Geological Society. Mr. Lawson was also one of the oldest members of the Spectacle Makers' Company, to whom he has left 100*l.* He had been twice Master. From August 1831 to August 1832 he kept a careful record of the solar spots, which he presented in a neat form to our Society.

In 1846 he published an account of his observatory (with drawings) under the title of *The Arrangement of an Observatory for Practical Astronomy and Meteorology.* This paper describes his achromatic refracting telescope of 11 feet focal length and 7 inches clear aperture — an instrument which Mr. Dollond boasted of as the finest he had ever made. It is supported on a polar axis, with declination and horary circles. This telescope is unusually well furnished with micrometers and eye-pieces up to the power of 1400, which the telescope fully bears. The telescope is driven by clock-work upon a novel construction, the maintaining power being a weight, and the correcting power paddles immersed in a basin of quicksilver. For the observation of zenith stars, Mr. Lawson contrived a convenient chair denominated a "Reclinea," for which the Society of Arts (of which he was a member) voted him their silver medal. The pamphlet next describes his 5-foot telescope, with a clear aperture of 3¾ inches, *twin* to the Rev. W. R. Dawes' fine instrument, and considered very perfect. This was mounted upon a stand of Mr. Lawson's own contrivance, called "The Journeyman," which had a vertical and horizontal movement, and being so constructed that at whatever height might be the object examined, the eye of the observer need not be moved. After alluding to the Magnetic Variation Transit, the pamphlet goes on to describe the different test stars examined, together with some observations of curious astronomical phenomena. He next describes the meteorological observatory, the construction of the "Atmospheric Recorder," "Thermometer Stand," and "New-Point Instrument." His 11-foot telescope, together with all the apparatus attached, was shortly before his death presented to the Royal Naval School of Greenwich; his 5-foot telescope to Mr. W. G. Lettsom, whilst the whole of his meteorological instruments, including the atmospheric recorder, Franklin's hygrometer (which was made by and belonged to that philosopher), the magnetic variation transit, two telescopes, and a number of meteorological books, to Mr. E. J. Lowe, to found a private meteorological observatory at Beeston, near Nottingham.

In 1845 Mr. Lawson read a paper of "Observations on the Placing of Thermometers, with the Plan of a Stand," at the Meeting of the British Association; and in 1846 presented a model of this stand to the Society of Arts, who awarded a prize for it. Plans of this thermometer-stand were subsequently published and distributed by Mr. Lawson. In 1847 he published

a brief *History of the New Planets*; in 1853, an account of two inventions of his called the "Lifting Apparatus," and the "Surgical Transferrer." The former is so contrived that upon being fixed to a bed, the patient may be lifted up by means of it without altering his recumbent position; the latter is adapted for moving the wounded without inflicting pain. In March 1855, he also published a pamphlet, *On the Advisability of Training the Youth of Britain to Military Exercises, as productive of National Safety.*

In December 1851, Mr. Lawson proposed to give the whole of his astronomical and meteorological instruments, together with 1050*l.*, to the town and county of Nottingham, provided a requisite sum of money could be realised, in order to build an observatory, and to endow it with 200*l.* a-year. A public meeting was called on the 13th of January, 1852, for the purpose of devising proper means in order to secure this noble boon both to science and the neighbourhood. A committee was formed, of which the Duke of Newcastle was chairman, and Mr. E. J. Lowe honorary secretary. This committee prevailed upon 727 individuals to subscribe. A sum amounting to 6562*l.* was collected, the Corporation of Nottingham voted land of the value of 600*l.*, and Government proposed to add 2000*l.*, making a total of 10,211*l.* A codicil was added to his will, at the request of the committee, and afterwards the instruments were conveyed to the Duke of Newcastle, in joint trust with Mr. Lawson, to secure the due fulfilment of the agreement. In this great undertaking, Mr. Lawson was ever ready to sanction the extension of time allowed, and other requests made to him by the committee. Unfortunately, however, when everything had been accomplished, the money valuation of the instruments was disputed, and differences of opinion arose, which ended in the return of all the subscriptions, and the abandonment of the plan.

Mr. Lawson's talent was not wholly confined to scientific pursuits, and many are the improvements he made in various domestic arrangements. He was both a philosopher and a philanthropist. Lamenting, as he did, the necessity of the present war, still he was quite alive to the national honour of his country, and strongly urged the advisability of every preparation being made to ensure the security of Great Britain. He was fond of microscopic investigations, and had a good microscope. Indeed, his house literally teemed with objects of interest, and he always took great pains to describe and explain, in a clear and perspicuous manner, either the wonders of the starry vault or of terrestrial objects. Mr. Lawson would spend hours together in his observatory with those who wished to learn more about the heavenly bodies. He also delighted to exhibit various specimens of minute objects with his microscope, and endeavoured to bring down his knowledge to the comprehension of his audience — a trait of real benevolence in a true philosopher.

Mr. Lawson's charity, without ostentation or publicity, was

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during his lifetime as unbounded as his love for science, and at his death he bequeathed to the Bath General Hospital 200*l.*; to the Bath United Hospital, 200*l.*; to the Walcot Dispensary, 200*l.*; to the Ear and Eye Infirmary, 100*l.*; and to the Bath Baths and Wash-houses, 300*l.* He had a large fortune, which he divided in his will among 139 persons.

The loss of all his papers prevents the possibility of describing the different inventions of Mr. Lawson. Amongst them, however, may be mentioned his " Solar Eyepiece," " Astronomical Reclinea," " Surgical Transferrer," and " Lifting Apparatus." In 1821, 1822, and 1823, he made daily observations on atmospheric electricity, and subsequently occasional records. He felt a great desire to spread a knowledge of the different branches of philosophy as widely as possible, and to promote a taste for science in young persons, which he endeavoured to encourage when once begun, and to this purpose much of his time was devoted. He died calmly on the 22d of August, 1855, in his eighty-second year, his clear-sightedness and kind disposition continuing to the last. His remains were deposited at Weston, on the 27th of August, in a vault containing many members of his family. It is painful at all times to record the loss of a friend, but one so deeply lamented, and whose many amiable qualities endeared him to all who were personally acquainted with him, will cause his memory long to be cherished and deeply felt.

WILLIAM DEVONSHIRE SAUL, who died April 27, 1855, at an advanced age, was in business as a spirit-merchant. He was a member of several scientific societies, and was greatly attached to knowledge, and much interested in its spread and progress. He was especially attached to geology, and has formed an excellent museum in Aldersgate Street. This he invited those who chose to inspect on a fixed morning in each week; and on these occasions he would give a descriptive lecture, and then encourage free conversation, giving and receiving information on his favourite subject. While travelling in the country on the business of his firm, it was his custom to take every opportunity of delivering lectures on geology. His museum is bequeathed to the Institution in John Street, Tottenham Court Road.

Mr. Saul was a regular attendant at our meetings until his death. He was a useful and upright member of society, and made it his business to advance all that he held good and useful. A dealer in spirits, he was a donor to Temperance Societies.

RICHARD SHEEPSHANKS was born at Leeds, July 30, 1794. His father was engaged in the cloth manufactory, and the son was intended to be in the same business. His earlier education, therefore, though of the liberal kind, was rather commercial, and arithmetical and other mathematical training was plentifully bestowed. At the age of fifteen he found out that the prospect of a mercantile life was not agreeable to him, and he wrote to inform his father of

his wishes. The father, without hesitation, gave up his own plan, and applied by the post of the night on which he received the letter to the late James Tate, of Richmond in Yorkshire, to know if he could receive his son as a pupil. Mr. Tate had at that time one of the highest reputations as a classical teacher, a reputation to which his scholars were continually adding. He, was, moreover, one of the kindest of men, and his social virtues and literary celebrity will often be alluded to, as one after another of his distinguished pupils claims the notice of the biographer. Between the teacher and the pupil the warmest feelings of affection grew up : and the closest friendship subsisted between them so long as they both lived. Under such tuition Mr. Sheepshanks soon made up deficiencies, and was placed at Trinity College, Cambridge, in October 1812. He took a wrangler's degree in 1816, and was elected a Fellow of his college in 1817, at the same time as his contemporary, Dr. Whewell, the present Master of the College, one of his oldest and closest friends. He applied himself to the study of the law, and was called to the bar in 1824. He intended to practise this profession, but was partly prevented, it is supposed, by a tendency to inflammation in the eyes, which accompanied him through life. It is, however, as likely that the death of his father, which placed him in easy circumstances, and his own increasing bias towards scientific pursuits, were the principal reasons of his declining to pursue the career for which he had fitted himself. In 1825 he took orders, at the same time as his friend, Mr. (afterwards Archdeacon) Hare; the two read and prepared themselves together. His attention to astronomy soon absorbed a great part of his time. He became a Fellow of this Society in 1825, and from that time was one of the most active in the administration of our affairs. He filled at one time the post of Secretary; but that of President he always refused. This fact must be stated in justice to the Council, who often, and especially after the death of Mr. Baily, pressed the presidency of the Society upon him. Among the services rendered by him to the Society, one of the most prominent is the elevation of the *Monthly Notices* to their present form. Before the change, these *Notices* were strictly abstracts of proceedings, and the possessor of the quarto *Memoirs* might almost dispense with them. Since the change they are, as we all know, supplements to the *Memoirs*, which are imperfect without them. In maturing this alteration he spent time, labour, and money. It was for some years his practice to print, at his own expense, an additional number of copies, which he distributed in his own way, to bring the work into greater notice and circulation. In fact, his pecuniary benefactions to the Society were more than most of the Fellows knew anything about. When the portrait of Mr. Baily, which is now in our meeting-room, was nearly completed, the funds for payment were found to fall considerably short of what was required, owing to a misunderstanding, which need not be further alluded to. Mr. Sheepshanks, who had no hand in creating the difficulty, quietly made up the

necessary amount, and never allowed what he had done to be known, except to a few of his most confidential friends.

In the summer of 1828 he joined Mr. Airy and Dr. Whewell in the pendulum operations which they undertook in Cornwall. The object of this operation, as is well known, was to determine the difference of gravity at the top and bottom of a mine, by simultaneous observations of the vibrations of invariable pendulums, and by the comparison of the clocks with which the pendulums were immediately compared. In planning the order of observations, Mr. Sheepshanks took an important part; and we have the testimony of the Astronomer Royal that it was to his energetic representations that the adoption of the laborious principle of incessant observations (to which the Astronomer Royal ascribes the success of his late repetition of the experiment) was entirely due. In the execution of the work Mr. Sheepshanks took charge of the upper station. The enterprise was frustrated, after various difficulties had been met and mastered, by an accident in the mine itself, which stopped works of all kinds, and finally caused a partial flooding of the mine.

In 1829 Mr. Sheepshanks was an active member of the Syndicate for establishing the regulations of various kinds by which the Cambridge Observatory is still governed. We need hardly say that he was an active member of the Board of Visitors of the Royal Observatory at Greenwich.

In 1832 he first interfered in a matter into the personal part of which there is no occasion to enter,—we mean the action brought by Messrs. Troughton and Simms against Sir James South, to recover payment for mounting his large equatoreal. This was, in truth, a scientific question, and it led to one of the closest discussions which an astronomical instrument ever underwent, before Mr. (now Mr. Justice) Maule as arbitrator. Mr. Sheepshanks was, though not nominally, yet actually, one of the counsel for Troughton and Simms, and the late Mr. Drinkwater Bethune,—a loss to science in England, and to education in India,—was counsel for Sir James South. The claim of the plaintiffs was awarded to them, after the sifting of a great deal of evidence; and it is to be regretted that the nature of the proceeding rendered it inexpedient to publish a discussion in which two men so acute and so well versed in their subject contended before a judge who was in both points in the same rank with either of the two. It may be noted that the first simple and efficient clock-motion applied to an equatoreal was that invented by Mr. Sheepshanks, and used in trying the instrument above alluded to.

In 1833 he was referred to by the Admiralty with respect to the edition of Groombridge's Catalogue which had been prepared and even printed. To his representations on this subject it is mainly due that an edition which would have fallen short of the reasonable expectations of astronomers was suppressed, and a fitting publication was prepared by Mr. Airy.

After the return of the expedition down the Euphrates under

Col. Chesney, with the loss of their excellent astronomer, Lieut. Murphy, the mass of astronomical observations was placed in the hands of Mr. Sheepshanks for reduction. This labour cost him much time and thought, but it involved a service to astronomy and a tribute to the memory of a friend ; and either of these circumstances would have commanded his best exertions.

Mr. Sheepshanks was a member both of the Commission of 1838 for considering the mode of restoring the national standards of length and weight, and of the Commission of 1843 for superintending the actual constructions. On the death of Mr. Baily, in 1844, he volunteered to undertake the re-construction of the standard of length. On this great work the last eleven years of his life were much occupied : and there is too much reason to fear that his days were shortened by the exertions which he made. The Fellows would hardly suppose, that in one of the cellars under their Apartments, while otherwise occupied in the business of the Society, and always ready to advise a young astronomer, or to pass an hour of pleasant gossip, Mr. Sheepshanks recorded micrometrical observations to the number of just five hundred short of ninety thousand. The account of the result, which received its legal sanction on the day following that on which Mr. Sheepshanks was struck by his mortal illness, will be published by the Astronomer Royal. But the following few sentences may serve the purpose of the present notice.

The first part of the work consisted in examining and selecting, among the existing measures of length, those which, having been carefully compared with the imperial standard, and being in other respects trustworthy, might be used as bases for reproduction of a standard identical in length (within the limits of uncertainty which the construction of the old standard allowed) with the imperial standard. Mr. Baily had first pointed out, and Mr. Sheepshanks more strongly urged, that it would be imprudent to rely so completely upon the permanence of the standard scale of this Society as they had at first intended to do; and that the Kater scale of the Royal Society, and the two 3-feet bars of the Ordnance Survey, though inapplicable to any other purposes of a scale, would probably be judged by the scientific world to be better adapted to the purpose of delivering down a single measure of length. The Superintending Committee assented to this opinion. The next work was, to arrange a comparing apparatus, which, on the score of mechanical firmness and freedom from the effect of changes of temperature, should present such a security for the accuracy of comparisons as had never been obtained before. How these advantages were gained, by use of the massive apparatus in the cellars of this Society, it is beyond our purpose to point out; but there cannot be a doubt that the comparisons made by Mr. Sheepshanks are so far superior to those of all preceding experimenters, including Kater and Baily, as to defy all competition on the ground of accuracy. What trials were made of different

methods of producing the terminal divisions of the measure,—different modes of illuminating, &c., it is impossible here to state; but in illustration of the unsparing labour which Mr. Sheepshanks was ready to bestow whenever the question of accuracy was raised, the following incidents may be mentioned. The first is that, when the bars intended for the new Parliamentary standard and its official copies had been, as was supposed, sufficiently compared, a new and hitherto unsuspected difficulty showed itself in the nature of personal equation. At once Mr. Sheepshanks rejected the mass of observations already made, and commenced a new series; availing himself of the services of those of his friends whose habits of observation and general accuracy made it probable that the combination of all their observations with his own, as repeated in the new series, would sensibly eliminate the personal equation; but charging himself with the labours of adjustment and of immediate computation of results. The second incident is, that in the spring of 1855 some circumstances led Mr. Sheepshanks to think that his " Generating Bar," on which he had relied mainly for the production of accurate copies of the standard, had undergone a small change, and he at once proposed to reject all the observations which had entirely occupied several years, and to commence *de novo*. The whole of the suspected change amounted, in its greatest apparent effect, to no more than the thermal change produced by $\frac{1}{10}$th of a degree of Fahrenheit. Ultimately, the grounds for suspecting a change so far disappeared that it was not thought necessary to repeat the observations.

Mr. Sheepshanks was actively engaged in 1838 in the chronometrical determinations of the longitudes of Antwerp and Brussels; and in 1844, of those in Valentia, Kingstown, and Liverpool. Some discussions which arose about the site of the Liverpool Observatory led him into controversy, the result of which was a pamphlet exposing the futility of the objections made.

Mr. Sheepshanks resided in town from 1824 to 1841 at a house in Woburn Place, where he fitted up a small observatory. He then removed to Reading, where he continued to reside, though constantly called to London by the business of the standard of length. He died at Reading August 4, 1855, of apoplexy, after a day or two of illness.

Mr. Sheepshanks was more than an astronomer. He had a taste—a family taste—for the fine arts. He retained to the last a love of and familiarity with classical authors, and he was well versed in modern European letters. He was especially attached to the old English dramatists, and was never at a loss to turn to a passage in any of them. From Shakspere, as the readers of his controversial pamphlets know, he could produce something to any purpose. To keep up and augment his classical learning was with him, for many years, a positive duty. He looked forward to becoming a Senior Fellow of his College; in which case he would have been required to take part in the examinations of candidates

for fellowships; and he determined to enable himself to fulfil this duty in a manner worthy of the reputation of the college as a school of philology. When he found that he was unlikely, from other occupations, to perform this duty, he determined not to accept his place in the seniority when it fell to him; and he accordingly remained one of the Junior Fellows till his death.

For history, especially political history, he had a strong taste; and the newspaper of the day was but the end of a long series, the whole of which he had studied. One of his pursuits was not a little remarkable. He had studied military tactics, ancient and modern, to an extent which was hardly credible to those who had no means of knowing it except ordinary report. He had read on the sub-ject from Polybius to Napier, and could speak on any marked campaign with readiness, and with an apparent precision which military judges pronounced real. We suspect that if there be any one in Britain who has studied *both* ancient and modern warfare to an equal extent with Mr. Sheepshanks, his name is not before the public in the credit due to the combination.

In public life he took no part except on one occasion. In 1831 he allowed himself to be nominated one of the commissioners for regulating the boundaries of the boroughs under the Reform Bill. In this character he visited and arranged most of the boroughs be-tween the Humber and the Thames. When associated with ano-ther, his colleague was the late Mr. Tallents, agent to the Duke of Newcastle, who, as might be supposed, was a strong Tory. Mr. Sheepshanks himself had been a thorough Liberal in politics from his youth. The two agreed to differ so well, that they contracted a warm friendship for each other.

As an astronomer, Mr. Sheepshanks especially devoted himself to the theory and history of the astronomical instrument; and his peculiar pursuit led him to know more of the history of astronomy in general than usually falls to the lot of the practical observer,— we mean, more of the original records of that history. The articles which he wrote on astronomical instruments for the *Penny Cyclo-pædia* are not yet surpassed in current utility, and were never surpassed in soundness and clearness. He was of a strong me-chanical turn, and has been heard to say, that if in his youth machinery had been applied as it is now, he might probably have acceded to his father's wishes, retaining the direction of the works, and leaving the buying and selling to his brothers.

He was, above all other Fellows of this Society, the adviser of the aspirant who desired to build an observatory, or to devote himself to any astronomical pursuit. For this he was especially qualified, not only by his familiarity with all the detail of the instruments, but by his leisure and by his sense of the duties which that leisure imposed upon him.

On his social character we may be permitted to quote an extract from the *Examiner* newspaper of the 8th of last Sep-tember; and the more so as the freedom of expression of the journalist will permit a more pictorial representation of our

departed friend than the Council might choose to make on its own account:—

"Any one who was in company with Mr. Sheepshanks for the first time would have remarked, and at first with some curiosity, a man of hardly middle stature, of rapid and somewhat indistinct utterance, of a very decided opinion upon the matter in discussion, and apparently of a sarcastic turn of thought and a piquant choice of phrase. By the time he ascertained, which he would not be long in doing, that the speaker was a scholar and a gentleman, he would at first be inclined to set him down as a very irreverent scholar and a very positive gentleman. This would last until a point arose on which Mr. Sheepshanks had not thought or had not read; and then his auditor would perceive that he had the not very common faculty of making a wide difference between his mode of talking about what he had attended to, and what he had not. His utterance would slacken, his energy of manner would abate, and he would resemble a cautious witness speaking upon oath. When it happened that the necessity arose of defending what he really respected against any opposition worth considering, the tone of flighty sarcasm disappeared, and an earnest deportment took its place.

"All this arose from a mixture of two prominent feelings: a strong, abiding, and self-sacrificing devotion to what he held good and true, and a keen, sarcastic, and laughter-loving contempt for all that pretended to be what it was not. No wonder that, in the world we live in, the second feeling predominated over the first in the formation of his habits of speech and of argument. With no lack of allowance for every well-meant and honest effort, his temperament did not permit him to work out an average from the head of gold and the feet of clay: the clay did not depreciate the gold, nor did the gold enhance the clay. No man knew better how to defend his scientific and personal opponents, on those points on which they were defensible; and no man more constantly did it. The remaining trait which must be noticed is the vigorous and practical character of his friendship : his active and unwearied assistance was as surely to be reckoned on as a law of nature, especially if to the cause of his friend was attached the opportunity of supporting some principle, or aiding some question of science. Nor was his kindness of feeling limited to his friends. It showed itself in real and thoughtful consideration for all with whom he came in contact. Had he been a physician, his fanciful and self-tormenting patients would have thought him the worst of their ills; his milder cases of real suffering would have been cheered by his bantering kindness; while severe and dangerous malady would have felt the presence of the sympathy which money cannot buy, shown with a delicacy which benevolence itself cannot always command."

The last of Mr. Sheepshanks's publications was a defensive pamphlet — or partly defensive — in answer to an imputation, to which we need not here allude further than by describing it as an

impeachment of his integrity, upon the evidence of a conversation alleged to have been held thirty years before it was brought forward with an eminent man who died twenty years before it was brought forward. Of course this sort of evidence never received the slightest attention from any of the scientific bodies before whom it was proposed for inquiry; nor would it have been mentioned here, public as the matter has become, except simply to record that sense of the utter needlessness of any reply to such an accusation, which the Council showed when they neglected the formal application made to them on the charge. The subject of this memoir lived in the regard and respect of all who knew what he was, and were unbiassed by the feelings which controversy too often creates. In this Society he must always be remembered with gratitude as an earnest friend, a laborious servant, an enlightened manager, and a conscientious administrator.

At the Royal Observatory of Greenwich, there is no important change or addition to be recorded. Indeed the recent developments of the organisation of that establishment, involving as they do the reduction to routine of the processes for connecting astronomy, magnetism, and meteorology, with the sciences of galvanic electricity and photography, and the surmounting of the various practical difficulties which are sure to attend such applications, have afforded ample employment to the Astronomer Royal and his staff of assistants, and have taxed the skill and industry of all connected with the establishment as much, if not more, than in any preceding year. While the ordinary routine operations of astronomy have been carried on without any abatement of the vigour of former years, the details of the operations by which time is transmitted to determinate stations along lines of railway and to the port of Deal have been watched with scrupulous care and anxiety; and such improvements have been from time to time effected as have been taught by the experience gained in the working of the system.

It was stated in the Report of last year that the dropping of the Deal ball automatically by means of a current sent primarily from Greenwich at the instant of the drop of the Greenwich ball, had come into regular operation at the beginning of the year 1855, and confident hopes were expressed that the drop of the ball at this port would be soon accomplished with all desirable regularity as a matter of routine business. This hope has been fully realised. The number of cases of failure, arising from causes of all kinds beyond the control of the Observatory, since the last Report, has been only sixteen; and during an interval of five months,—namely, from the middle of June to the middle of November,—there were only two failures. The failures of the galvanic apparatus within the precincts of the Observatory have been very few indeed, and the whole of it is completely under easy control of the assistants charged with the management of it.

Though time has been transmitted throughout the year 1855

to those stations only which were specified in the last Report of
this Society, yet arrangements have been made for extending the
benefits of the system to the public on a still larger scale. In com-
pliance with a request urged by the Post-Office authorities, the
Astronomer Royal has succeeded in negotiating with the Electric
Telegraph Company for a line of wire, and in devising the me-
chanical measures necessary for putting into galvanic connexion
with Greenwich four clocks at the General Post-Office at St. Mar-
tin-le-Grand, and one clock at the Office in Lombard Street, with
the object of supplying to those important establishments accurate
Greenwich time. If no unexpected obstacle should intervene, we
may expect that this desirable object will be shortly accomplished.
If the galvanic communications with the Post-Offices should suc-
ceed to the extent confidently anticipated, it is not improbable that
the system may be extended so as to include the Houses of Parlia-
ment, the Admiralty, and such other of the public offices as may
find it desirable or convenient.

With regard to photographic manipulations, the routine opera-
tions now in use give almost invariably excellent delineations of
the movements of the magnets, and of the dry and wet thermo-
meters, and the barometer; and nothing more seems to be desired,
either with regard to the cleanness and delicacy of the photo-
graphic sheets, or to the distinctness of the traces on them. It has
been thought, therefore, a desirable object to obtain a considerable
number of copies of each of these magnetical and meteorological
records; and for this purpose, considerable time and thought have
been expended in printing off as many as the force of assistants
attached to this department would permit, the bright sunshiny
days being devoted chiefly to this purpose. Though, however, by
this means a great many valuable secondaries and tertiaries of
the original traces have been obtained, still it appears that the
resources of the Observatory are not sufficient to produce with
regularity that number of copies which the interests of science
seem to require, and the Astronomer Royal has proceeded to make
preliminary inquiries concerning the expense which would be
incurred by the printing of a definite number of copies by a pro-
fessional photographer.

Thus, though the operations of the last year at Greenwich are
not characterised by any actual novelty, yet enough is exhibited
of its steady and vigorous action and of its preparation for still
greater extension of its organisation and usefulness, to give evi-
dence of its vitality, and of its attention to the highest and best
interests of science.

On closing the observations for the Circumpolar Catalogue, it
had been Mr. Johnson's intention to have applied the instruments
of the Radcliffe Observatory to a revision of Piazzi's Catalogue, on
the same plan as he had pursued with Groombridge's. However,
after having made some preliminary arrangements for this purpose,
it seemed to him that the amount of observation and reduction it

would entail would be more than the personal resources of the establishment could well bear, while at the same time engaged in preparing the Circumpolar Catalogue for the press. He has, therefore, relinquished this scheme, for the present, for the less laborious task of constructing a catalogue containing all known objects among the fixed stars remarkable for physical or systematic peculiarities; including under these heads,—

1st. Stars of conspicuous brilliancy to the 3d magnitude inclusive.

2d. All stars known to be, or suspected of being, variable.

3d. Stars remarkable for colour.

4th. Stars having proper motion amounting to $\frac{1}{10}$th of a second of space.

5th. Double stars known to be affected by orbital motion.

There is no existing collection in which all these objects are to be found, therefore the proposed catalogue, in addition to its value as such, will also furnish a useful index for reference to persons interested in researches connected with such objects. Care will be taken to introduce all new discoveries coming under the forementioned heads.

The first portion of this new work will appear in the Fifteenth Volume of the Radcliffe Observations, which will be published in the course of a few weeks. This volume will also contain the first specimens of photo-meteorographic registrations, which are being carried on regularly at that establishment.

The Heliometer has been employed almost exclusively in parallax researches. *Arcturus, Castor,* and *α Lyræ,* have been the principal objects of investigation, but as none of the series were completed at the end of 1854, the year comprehended in the new volume, Mr. Johnson has thought better to reserve the publication of the Observations until he is able to present them complete.

The Cambridge Observatory has now been under the direction of Professor Challis for twenty years, and during that time observations have been continued with scarcely any interruption. The hardest portion of the work is the keeping up the publication of completely reduced observations. A reduced catalogue of the stars, chiefly zodiacal, observed in 1850, has been completed, and additional errors in the catalogues of Lalande, Weisse, and the British Association, have been discovered.

The apparatus for eliminating from the transit observations the effect of the irregularity in the forms of the transit pivots has been used again,—this having been now become an annual operation. The results obtained are completely confirmatory of those of former trials, and of the exactness of the method. As a general result of these trials, it may be stated that the deviation of the pivots from the cylindrical form affects observed R.A. differentially and to a very small amount, and that the total amount of the error takes effect only in a determination of the longitude of the observatory.

Professor Challis has recently adopted the following method of eliminating the effect of flexure in observations with the mural circle.

Two collimators are mounted on a wooden stand in such a manner that, being carried on two arms about a horizontal axis nearly coincident in direction with the axis of the circle, they can be made to collimate with the circle telescope, and with each other, for any zenith distance. This method has already been applied in other observatories for determining the amount of flexure in the horizontal positions of the telescope; but this is the first time that it has been extended to all positions. The apparatus has been so arranged that when the collimators are mounted, they do not seriously interfere with the use of the circle in daily observations. The collimators have been employed for determining the effect of flexure in only two positions of the telescope up to the present time, the zenith distances of $90°$ and $45°$. In the former the amount of flexure is found to be not less than $4''$, and in the other little more than $1''$, which is somewhat larger than the amount indicated by the direct and reflexion observations of stars. Probably by longer experience in the use of the collimators, greater precision will be attained and the above results be modified.

The object proposed to be effected by the use of this apparatus is to ascertain the law which the flexure follows from $0°$ to $90°$ of zenith distance, and thus to eliminate the effect of flexure from the observations of north polar distances more completely than is practicable by the direct and reflexion observation of stars. Mr. Challis adds as follows:—

" Since June last, I have proceeded further with the experiment for finding the effect of the flexure of the Circle Telescope on observations of N. P. D. The results of the first trials were found to be unsatisfactory, owing to the support of the collimators not being sufficiently steady. I have had the support placed on a firm basis of brickwork, surmounted by a stone slab; and, acting upon a hint given me by the Astronomer Royal, I have provided means of clamping the extremities of the arms which carry the collimators. The results of trials made since these arrangements have been very consistent, and have shown that the amount of horizontal flexure, mentioned in the Report of the Syndicate, is much too large, the actual amount being certainly below $1''$. I have not yet completed a series of determinations for different altitudes."

In the usual Report to the Observatory Syndicate, Professor Challis gives a detailed account of the observations made during the year. Mr. W. T. Lynn has succeeded Mr. Criswick (removed to Greenwich) as Junior Assistant.

At Liverpool, Mr. Hartnup has observed all the planets and comets that have been discovered during the past year. Most of the observations have been printed in the *Astronomische Nachrichten*.

In lunar photography considerable progress has been made; Mr. Crookes, the celebrated chemist and photographer who assisted Mr. Johnson at Oxford, being in the neighbourhood, visited the Observatory for a few days, and with his assistance they succeeded in taking a good collodion negative of the full moon in the short interval of *four seconds*. Previous to this, Mr. Hartnup had never succeeded in obtaining a picture of the full moon in less time than from twenty to thirty seconds. Mr. Crookes has taken several collodion negatives to London with him, with the view of enlarging them and printing from them. If the small pictures cannot be enlarged satisfactorily, there would probably be quite sufficient light to produce a picture of five or six inches diameter, or even larger, direct from the telescope, with chemicals so sensitive as those prepared by Mr. Crookes. There is no difficulty in making the telescope follow the moon perfectly for two or three minutes.

The chronometrical expedition of the American United States Coast Survey was again renewed last summer. Mr. Sidney Coolidge, accompanied by one of the assistants of the Cambridge U. S. Observatory, came over three times with upwards of fifty chronometers. Mr. Coolidge brought out one of Mr. Bond's Spring Governors with him, and the transits were observed and the chronometers compared by the American galvanic process, both at Liverpool and at the Cambridge Observatory, by Mr. Coolidge. This result will therefore be free from the effects of personal equation.

The Board of Trade have supplied the Observatory with apparatus for testing barometers and thermometers, and Mr. Hartnup finds that it is of but little use giving captains the rates of their chronometers as dependent on the temperature, unless they are furnished with more accurate thermometers. An error of four or five degrees is quite common, and Mr. Hartnup recently tested a fine-looking thermometer, fitted up for taking the temperature of water at different depths, for a merchant captain, which stood at $80°$ when his standard read $88°\cdot5$. It is no uncommon thing to find barometers from half an inch to an inch wrong in some part of the scale between 28 and 31 inches. He tested one a short time ago which had the following corrections for scale reading : —

in.	in.	in.	in.	in.	in.	in.
At 28·0	28·5	29·0	29·5	30·0	30·5	31·0
— 2·22	—1·88	—·73	—·30	—·02	+·33	+ 1·07

The Annual Report for the past year has not yet been printed; but it will be seen that the Observatory maintains its character for amount of work done.

At his observatory at Redhill, Mr. Carrington has, during the past year, been making unimpeded progress in the two subjects to which his attention continues to be directed, namely, the observa-

tions of stars within 9° of the North Pole, and the forms and positions of the solar spots.

The region within 4° of the pole was, as related in the last report, under observation in the year 1854, and was very nearly finished in that year. During the past year the few observations still wanting were obtained, the reductions of the whole to apparent places finished, and the corrections to mean place, 1855·0 computed, but not applied.

In the next sub-zone of 4° to 7°, which was the intended work of 1855, 4540 observations were made, exclusively of observations of stars for the determination of instrumental and clock errors, being three observations apiece of 1514 stars, with two sole exceptions. The observations of this sub-zone are concluded, the reductions to apparent places within a fortnight of completion, and the corrections to mean place about one-third computed.

In the third and last sub-zone of 7° to 9°, about 1050 observations were obtained in 1855. It is expected that this sub-zone will be finished in September next.

In his second subject — the solar spots — Mr. Carrington has had a very light year, the reduction of all the observations obtained in 1855 occupying less than a fortnight. The sun was viewed on 227 days, on 150 of which the surface was found to be blank. The observations of the nuclei and detached spots seen on the remaining 57 days amounted in all to 135 only, the whole of which are finally reduced and diagrammed on the same system as was pursued in the former year. At that stage they are left for the present.

In a recent number of the *Monthly Notices* mention was made of a part-volume received from the Observatory of Harvard College, Massachusetts, containing observations of small stars in the neighbourhood of the Equator made with the great Cambridge refractor. The contribution is one towards the filling in of the details of the starry heavens; fourteen new stars being given for every one previously observed. A comparison might accordingly be naturally made between the present work and the volumes which have emanated from the Observatory of Mr. Cooper of Markree Castle; but in this, as in most other astronomical undertakings, diversity of method will be expected and found to exist, arising from the individual motives of the observers in selecting their fields of labour. Mr. Cooper's observations have been taken in the region of the Ecliptic by means of a frame of bars with an accuracy sufficient for the purpose of forming improved maps; while Mr. Bond has confined himself to a ribbon of stars on the Equator observed and reduced with a degree of refinement and an arrangement in the publication, especially aimed at the discovery of planetary or proper motion in the objects observed. The preface to these observations is an instructive one, and well worthy the attention of the practical astronomer. We might almost say that the probable errors were over-discussed, were not the results the

first obtained by the new method of galvanic registration. We are much gratified also with this volume as testifying to an increasing recognition of the principle that a star once observed is a star incompletely and insufficiently observed for most of the purposes for which after-reference will be made to its recorded position. Although Mr. Bond has not rigidly followed out the second observation of every star in his zones, he has done so in as many instances as his method of sweeping his ground twice with a fixed telescope has practically allowed of. If it were suggested that an increase of breadth of the zones, and a limitation to the stars of the 10th magnitude, coupled with a strict adherence to the rule of observing every star down to the 10th magnitude twice, would be more immediately useful; it would be a sufficient reply that Mr. Bond has an instrument which sets its own conditions in some respects, and that these are partly inconsistent with what might be our wishes. The suggestion might be aptly made to those who are unfettered in the choice of an instrument for work of this class; and our unqualified thanks are due to Mr. Bond for his valuable contribution as it comes before us.

We have the pleasure of announcing that the First Part of the Volume of which the observations now commented on form the Second Part, is already in the press, and will contain an account of the circumstances which led to the foundation of the Observatory, and the observations of *Saturn* in the years 1848 to 1856, illustrated by numerous engravings. Letters from the Messrs. Bond also inform us of the completion of a second volume of zone observations, and add, that the printing fund by which these his first results have been given to the world is an endowment by private bequest of a permanent character.

After the decease of Mr. Sheepshanks, the Astronomer Royal undertook the examination of the papers relating to the comparisons of Line-measures or Standards *à traits*. It was found that, besides the five standards which, under the title of National Standard and Parliamentary Copies, had been deposited in various offices of the Government, there were more than forty well-compared and disposable copies nearly ready for distribution. Under the superintendence of the Astronomer Royal these have been numbered, engraved, and packed up, and thirty complete sets of British Standards (every set including a bronze copy of the yard-measure, and a gilt-bronze copy of the avoirdupois pound weight, and some including, in addition, an iron or steel copy of the yard measure,) have been distributed to our colonies and to foreign states.

The troublesome work of forming End-measures, or Standards *à bouts*, has been intrusted to Mr. Simms, acting under the general superintendence of Professor Miller. Several bars are finished with hard stone-ends, and are otherwise prepared; but none are yet actually compared with the fundamental line-measures.

In the autumn of 1854, the Astronomer Royal, assisted by a staff of six observers, made a series of pendulum experiments in the Harton coal-mine, near South Shields, for the determination, in the first place, of the difference in gravity at the top and bottom of a mine, and, in the second place, of the mean density of the earth; completing, in fact, an experiment which, in conjunction with Dr. Whewell, he had commenced twenty-eight years before in the Dolcoath Mine of Cornwall. The reduction of the mere pendulum experiments was effected in a few months, but much yet remained to be done. The country was to be surveyed, so as to give means of computing the theoretical attraction of the ground, and the specific densities of the rocks were to be investigated. The Corporation of South Shields gave directions for the survey; the owners of the mines supplied specimens of the rocks; and Professor W. H. Miller, of Cambridge, undertook the troublesome work of ascertaining their specific gravities. The whole of the computations have now been effected, and the result is, that the mean density of the earth is 6·57. This result, it will be re-marked, considerably exceeds those obtained by Mr. Baily and Mr. Reich, from repetition of the Cavendish experiment, as the latter exceeded that obtained by Drs. Maskelyne and Hutton from the Schehallien experiment. In the opinion of the Astronomer Royal, the new result is entitled to compete on at least equal terms with the old ones.

Astronomy is indebted to Archdeacon Pratt for a valuable investigation of the effect produced upon the direction of the plumb-line by the attraction of the Himalaya Mountains and the elevated regions beyond them. The operations connected with the measurement of the great arc of India had established beyond doubt the existence of a sensible disturbance arising from this cause. Thus, the amplitude of the northern division of the arc included between Kaliana and Kalianpur, when determined by astronomical observations of latitude at the two extreme stations, was found to be *less* than the value obtained geodetically, which ought to have been the case if the attraction of the elevated region in question exercised a sensible influence. The difference of the results amounted to 5″·236, but it still remained to ascertain whether this represented the exact effect of the disturbing forces to which it seemed to be attributable. Archdeacon Pratt has computed the effect of mountain attraction in this case by a skilful use of the most trustworthy data available for such an inquiry, and he has found that it considerably exceeds 5″·236, even upon the most favourable supposition which can be made for diminishing its value. His final conclusion is, that the Indian arc is in reality somewhat more curved than it ought to be, upon the assumption of the generally-admitted mean value of the ellipticity of the earth; but that, when the effect of mountain attraction is taken into account, the deviation from the mean ellipticity is *less* than when that effect is neglected in the proportion of 5 to 9. The

Astronomer Royal has suggested an explanation of the discordance between the result of Archdeacon Pratt's investigation and the quantity brought to light by the Indian Survey, which does not suppose any irregularity in the curvature of the earth's surface, but refers the discordance to the neutralising effect produced by the partial subsidence of the mountainous masses on the earth's surface into the heavier fluid underneath. It may be difficult to ascertain beyond all doubt the origin of the discordance; but it will be generally admitted that all speculations of this nature, when conducted upon sound principles, have a tendency to exercise a salutary influence upon the progress of science.

Since the last Anniversary of the Society the group of minor planets has received an accession of five new bodies, viz. *Circe, Leucothea, Fides, Atalanta,* and *Leda.*

Circe was discovered by M. Chacornac at the Imperial Observatory, Paris, on the 6th of April. Its period is about 1812 days.

Leucothea was discovered by M. Luther at the Observatory of Bilk, on the 19th of April. Its period is about 1865 days.

Fides was discovered by M. Luther, at the Observatory of Bilk, on the 5th of October. Its period is about 1580 days.

Atalanta was discovered also on the 5th of October by M. Goldschmidt at Paris. Its approximate period is 1683 days.

Leda was discovered by M. Chacornac on the 12th of January, 1856, at the Imperial Observatory of Paris. Its period appears to be nearly 1660 days.*

Three new comets have been discovered in the course of the past year.

The first comet was discovered by M. Schweizer, at Moscow, on the 11th of April. It was a faint telescopic object, which continued visible for a few weeks. The orbit has been found to be sensibly parabolic.

The second comet of the past year was discovered by Dr. Donati, at the Observatory of Florence, on the 3d of June. It was also discovered independently on the following evening by M. Dien, at the Imperial Observatory, Paris, and by Dr. Klinkerfues, at the Observatory of Göttingen. Dr. Donati has deduced elliptical elements from the observations of this comet, indicative of a period of 492 years. He has shown that there are some grounds for supposing it to be identical with a comet which appeared in the year 1362.

The third comet of the past year was discovered by M. Bruhns, at Berlin, on the 12th of November. It continued visible till about the end of December. A parabolic orbit appears to satisfy the observations.

* Since the Report was sent to press, intelligence has been received of the discovery of another minor planet, by M. Chacornac, at the Imperial Observatory, Paris, on the 8th inst., the evening of the anniversary. The total number of minor planets now amounts to thirty-nine.—ED.

The favourable position which the planet *Saturn* has recently occupied for observing the breadth of his rings has not failed to attract the attention of several observers. Mr. Main having submitted to a searching discussion a series of observations of the rings, executed by himself in recent years with a double-image micrometer, has arrived at the conclusion that there exist no real grounds for the hypothesis of M. Otto Struve, that the bright rings are gradually approaching the body of the planet. Professor Kaiser, of Leyden, has deduced a similar result from a discussion of various ancient and modern observations of the rings. Professor Secchi's observations would seem to indicate that the rings, besides having a rotatory motion around the planet, are also elliptical, a view of their constitution which the observations of Mr. De La Rue tend in some degree to support.

The *Astronomische Nachrichten* still continues to be conducted under the able superintendence of Dr. Peters. The Council have on various occasions deemed it expedient to direct the attention of the Fellows of the Society to this invaluable journal, an acquaintance with the pages of which is indispensable to all who would desire to know the state of astronomical science in the present day. From the liberality with which its columns are thrown open to contributions in every language, it has acquired a character of cosmopolitanism which no other scientific journal can aspire to; but while this circumstance serves materially to enhance the value of the *Astronomische Nachrichten* as a medium of communication between astronomers throughout the whole civilised world, it must be admitted, on the other hand, that it constitutes a claim to the support of that journal, not merely on the part of professional astronomers — who are deeply interested in its maintenance and will always be glad to contribute towards that object — but also on the part of the amateur astronomer, who finds so much useful instruction in its pages, and of that large class of individuals who, without positively devoting any portion of their time to astronomical pursuits, enjoy delight in contemplating the grand truths of our science, and are always willing to contribute towards its advancement by any means that may be pointed out to them as efficacious and practicable.

Papers read before the Society from February 1855
to February 1856.

1855.
Mar. 9. Observations of Comet II. 1853. Mr. Maclear.
     Appearance seen in the Moon. Mr. Hart.
     On the Orbit of 70 *Ophiuchi.* Mr. Fletcher.
     Account of Astronomical Operations in the Colony of
        Victoria. Mr. Ellery.
     Note on Astronomical Refractions. Sir John Lubbock,
     Observations of Comet I. 1855. Dr. Donati.
     Description of an Observatory. Mr. Jeans,

On the Orbits of *α Centauri* and *σ Coronæ Borealis*. Capt. Jacob.

On the Moon's Parallax. Mr. Sang.

April 13. Note on Astronomical Refractions. Sir John Lubbock.

On Observing the Positions of Solar Spots. Mr. Carrington.

On the Theory of Foucault's Gyroscope Experiments. Rev. B. Powell.

May 11. Prospectus de l'Instrument dit Indicateur des Variations. M. De Kleinsorgen.

Discovery of a Planet.　M. Chacornac.

Do.　　do.　　M. Luther.

On a Phenomenon seen in the Planet *Venus*. Rev. W. R. Dawes.

Note on the Measured Distances of 70 *Ophiuchi*. Rev. W. R. Dawes.

On the Galvanic Determination of the Longitude of Fredericton. Prof. Jack.

Report on an Occultation of *Venus*. Mr. Ferguson.

June 8. Suggestion respecting the Probability of Discovering Planets in the Vicinity of the Sun. Dr. Dick.

On the Determination of Orbits of Double Stars. Capt. Jacob.

On the Difference of Longitude between Cambridge, U.S., and Liverpool. Mr. Bond.

Note on the Construction of Telescopes. M. Sturm.

On the Physical Cause of the Rotation of the Planets. Mr. Nasmyth.

On the Constants of Nutation and Aberration, &c. Rev. R. Main.

Nov. 9. On the Difference of Longitude between Halifax and Cambridge, U.S. Commander Shortland.

Observations of Comet II. 1855. Mr. Hartnup.

Note on certain Anomalies in the Motion of 70 *Ophiuchi*. Capt. Jacob.

Discovery of a New Comet. M. Luther.

Observations of *Atalanta* and *Fides*. M. Rümker.

Planetary Observations. M. Santini.

Account of Recent Astronomical Operations in Russia. M. O. Struve.

Positions of 20 Polar Stars as determined at Redhill. Mr. Carrington.

Dec. 14. On Certain Cases of Personal Equation. The Astronomer Royal.

Occultations observed at Highbury. Mr. Burr.

Note on the Probable Occultation of Stars by *Saturn*. The Astronomer Royal.

On the Zodiacal Light. Mr. Lowe.

On the Dimensions of the Rings of *Saturn*. Rev. R. Main.

1856.

Jan. 11. Description of an Obxxxxx Equatoreal and Stand. Rev. S. King.

Observation of *Venus* near Conjunction.   Mr. Brodie.
Measures of *Saturn*.   Mr. De La Rue.
Note on the Occultations of *Antares* during the year
   1856.   The Astronomer Royal.
Observations of the Planet *Saturn*.   Mr. Lassell.
Note on Solar Refraction.   Prof. C. P. Smyth.
Discovery of a new Variable Star.   Mr. Hind.

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*List of Public Institutions and of Persons who have contributed
   to the Society's Library, &c. since the last Anniversary.*

Her Majesty's Government.
Royal Society of London.
Royal Society of Edinburgh.
Royal Geographical Society.
Royal Asiatic Society.
Royal Institution.
Royal Irish Academy.
Geological Society.
Linnean Society.
The Photographic Society.
Ethnological Society.
The Philosophical Society, Liverpool.
The Philosophical Society, Manchester.
Historic Society of Lancashire.
British Association.
United Association of Schoolmasters.
Institute of Actuaries.
Corporation of Glasgow.
Hon. East India Company.
The Registrar-General.
The Superintendent of the Nautical Almanac.
The Radcliffe Trustees.
The Art-Union of London.
L'Académie Impériale des Sciences de l'Institut de
   France.
L'Académie des Sciences de Dijon.
Le Bureau des Longitudes.
Le Dépôt Général de la Marine.
The Imperial Academy of Vienna.
The Imperial Observatory of Vienna.
Royal Academy of Munich.
The Royal Observatory of Munich.
Royal Academy of Berlin.
Royal Academy of Brussels.
Museo di Fisica, Florence.
The American Philosophical Society.
The Smithsonian Institution.
The Franklin Institute.
The Observatory at Harvard College.

The Observatory at San Fernando.
The University of Göttingen.
The Calcutta Public Library.
The New Orleans Academy of Sciences.
The Editor of the Athenæum Journal.
The Editor of the Literary Gazette.
The Editor of the Critic.

G. B. Airy, Esq.
Dr. Alexander.
Lieut. Ashe.
C. Babbage, Esq.
Prof. A. D. Bache.
Sir Edward Belcher.
M. Biot.
George Bishop, Esq.
W. Bollaert, Esq.
W. C. Bond, Esq.
M. F. Brünnow.
A. Caley, Esq.
Rev. T. Chevallier.
E. J. Cooper, Esq.
Prof. De Morgan.
M. De Verneuil.
Dr. Donati.
S. M. Drach, Esq.
Prof. Encke.
M. Fedorenko.
Dr. B. A. Gould.
W. Gravatt, Esq.
M. Grunert.
J. Herapath, Esq.
Rowland Hill, Esq.
J. R. Hind, Esq.
E. Hopkins, Esq.
M. K. Hornstein.

Capt. Jacob.
W. H. R. Jessop, Esq.
M. C. Kerlin.
Dr. J. Lamont.
M. Lartigue.
Dr. Lee.
M. Lindelof.
M. Mathieu.
L'Abbé Moigno.
Don C. Moesta.
M. Nobile.
M. W. Oeltzen.
J. Page, Esq.
H. Perigal, jun., Esq.
Dr. Peters.
M. Plantamour.
A. W. Price, Esq.
Prof. Quetelet.
J. Riddle, Esq.
Admiral Sir John Ross.
Signor Santini.
Capt. Shadwell.
C. K. Smith, Esq.
M. W. Struve.
M. O. Struve.
Thos. Tate, Esq.
R. Taylor, Esq.
M. Weisse.

Address delivered by the President, Manuel J. Johnson, Esq. M.A.,
on presenting the Gold Medal of the Society to Mr. Grant.

I rise, Gentlemen, according to custom, to say a few words on
the subject of the Medal which, as you have heard, your Council
has this year awarded to Mr. Robert Grant, for his book entitled
A History of Physical Astronomy,—an award so far remarkable,
that it is the first which, during the Society's thirty-six years'
existence, has been conferred on literary service.
No one, I am sure, who has looked into this book will dispute
its merits, or its high claims to the Society's ████████████ only

question that can be raised is, whether it comes within the sphere
of that kind of approval which your Council has thought fit to
express; whether, in fact, this bestowal of the Medal is not a
departure from the traditions of the Society, which has hitherto
confined the distinction to services technically astronomical in
theory or practice.

A reference to our bye-laws, however, will, I believe, imme-
diately dispel such objections. Therein your Council is enjoined
to make its award in favour of whatever work shall appear to it
most conducive to promote those ends for which the Society
was established; and whether our Founders contemplated such
an award as the present or not, certain it is they placed no
restriction on the action of their successors. Our Founders were
men of large experience; they knew the inexpediency of binding
down one generation to the maxims of another. They knew how
variable were the phases of human institutions and of human
pursuits, and how ephemeral circumstances must regulate the
conduct of public bodies.

However, Gentlemen, I cannot believe that our Founders ever
thought of excluding literature from this most emphatic expression
of your gratitude and respect. Can we suppose such a thought
to have entered the minds of Baily or of Colebrooke, men eminent
for their knowledge in this department of our subject? We are
hardly left to conjecture. Time, it is true, has thinned the ranks
of those who participated in the early struggles and triumphs of
the Society. However, happily there are still among us men who,
if not to be classed among our founders, were among our earliest
and staunchest supporters, and whose congeniality of mind with
our founders we know from the intimacy which once united
them. Gentlemen, we are still proud of our Herschel, our Airy,
our Smyth, and our De Morgan, whose sentiments we know.
Would I could add to this list, endeared to us by many asso-
ciations, another name,— that of Richard Sheepshanks, whose face
and form only a few short months ago were so familiar to us
all, whose sentiments, too, we know as well as if he were still
among us. No, Gentlemen, it has not been from any depreciation
of the value of literature, or of the accomplishments required for
its successful prosecution, or of the direct practical assistance
it renders to the astronomer in the closet and in the observa-
tory, that hitherto you have made no award in its favour, but
simply because you have not had an opportunity of doing so.

It seems, at first sight, strange, that in a subject like astro-
nomy, of which the ramifications are so wide, such should be
the case; but I believe it is in this very circumstance that we
find an explanation of the deficiency. The materials for his-
tory are so abundant, and the labour of research is consequently
so appalling, that, to say nothing of the rare mental qualifications
which the task demands, there are few men who have the leisure
or courage to encounter it. Then it happens in scientific as in
civil history, that during-particular periods, its events cluster

round certain individuals, who thus offer themselves as appropriate subjects for special histories. Accordingly, we find the great mass of astronomical history written in the form of biographies, which, however instructive, must be incomplete, and therefore, seldom calculated to produce that influence on science which is the great object of general history.

Except the work before us, I know of no other which has appeared in our own day approaching the character of a general history, though only for a short period, but Mr. Airy's Report on Astronomy to the British Association in 1832, — a document containing a fund of information, and exhibiting an amount of reading, research, and thought, very inadequately represented by the number of pages within which it is compressed. In an assembly such as I have the honour to address, it is needless to dwell on the direct benefits which flow from works of this kind. They are the maps of knowledge; they show the sources of those mighty streams which irrigate the plain of human civilisation; they show the barriers which obstruct their course ; they show where human skill and perseverance have opened channels which Nature seemed to deny. They mark the boundaries of the garden and of the wilderness, —the waste land which Science has been permitted to reclaim, and that whereon man, in humble dependence on his Maker, may still toil with industry and hope. Gentlemen, I must apologise for intruding reflections which will have suggested themselves to you much more forcibly than any language of mine can convey. I was going to say, that if illustration were wanting of the practical influence of such works, I know of none to which I could appeal more confidently than to that Report of the Astronomer Royal. There you will find a concise statement of the weak points of our science, both theoretical and practical, and a bold enumeration of the defects of English astronomy at the time it was written. If called upon now for a similar contribution, the author would have much to modify on the latter score ; but as he has been himself the principal corrector of the evils which he exposed, exception may be made to this illustration. However, there was one important subject mentioned in that Report to which especial attention was called, —viz. the anomalies in the orbit of *Uranus;* and we have, as you know, Mr. Adams's own authority for saying that it was this passage which first called his attention to the subject, and thus it has been a means of securing to England a large share in one of the greatest astronomical discoveries of any time.

It is not only in our own, but in European literature generally, that there is this scarcity of comprehensive histories of astronomy. There are many partial histories, such as those by Cassini, Bouillaud, Weidler, Riccioli, Costard, Lalande, Narrien, and others ; but the only work, of which I am aware, at all comparable with that before us in scope and treatment is *L'Histoire de l'Astronomie Moderne* of Bailly, whose tragic end forms so strong a contrast to the philosophic dignity of his early career. This work, contained in three quarto volumes, treats of the history of astronomy from the foundation of the school of Alexandria to the year 1781,

about the time when Laplace had established his reputation as a geometer, and was entering on that course of brilliant investigations which has rendered his name illustrious. The pupil of Clairaut, whose portrait he has drawn in a very fine passage of his *History*, Bailly may be said to have been contemporary with the first developments of the Newtonian philosophy,—in which, indeed, he took a part by his investigations of the theory of *Jupiter's* satellites. A scholar, a geometer, an astronomer by taste, a most eloquent writer, and an unaffected philosopher, he possessed many essential qualifications for an historian; and his book will always hold its place, not only for the information it contains, but as a model of style and temper. Bailly, however, had a double object in view; he wrote for the educated public as well as for astronomers, therefore he was necessarily less critical than he would otherwise have been. This work is, I believe, generally accompanied by two other volumes, *L'Astronomie Ancienne* and *L'Astronomie Indienne*, in which some whimsical views are propounded touching an antediluvian people, from whom he supposed we derived our astronomical knowledge; "who," as Delambre wittily observes, "told us everything but their name and where they lived."

These views, however, which really occupy but few passages in the works in which they appear, are not obtruded at all in *L'Astronomie Moderne*, where I have seen nothing inconsistent with the most sober judgment. Bailly's work, as I have said, terminates with 1781. In 1810, a very carefully-written continuation of it was published by Voiron, giving a concise account of the discoveries of Lagrange and Laplace up to that time.

Delambre's well-known history (the first volume of which appeared in 1817) is of another kind: it is rather a history of the astronomical processes for investigating the movements of the celestial bodies, than of the theory of gravitation. He devotes a section in his last volume (a posthumous publication, which appeared in 1827) to Newton; no more, however, than incidental mention is made of such men as Euler, D'Alembert, or Clairaut. His taste led him to practical astronomy in its most comprehensive sense, by which I mean not so much the manipulation of instruments as processes. Calculation was his delight, and the application of theory to practice. No one will dispute the immense value of a work containing the vast amount of information which this does on almost all astronomical subjects, or deny the service its author has rendered to his favourite science.

I have mentioned, I believe, the only works which had any claim to be styled standard histories in either branch of astronomy at the time Mr. Grant's book appeared, and they were becoming antiquated.

Fortunately it so happened that in 1847, just twenty years after the appearance of Delambre's last volume, Mr. Baldwin, a gentleman whose name must always be mentioned with honour in every assembly of educated Englishmen, as the enterprising publisher of that series of works known as the Library of Useful

Knowledge—which, more than any other, was the means of rousing public attention to the importance of scientific studies— in 1847, Mr. Baldwin being engaged in publishing a continuation of the series I have mentioned, Mr. Grant, then a young man, almost unknown to the scientific society of London, proposed to write for him a short history of Physical Astronomy. The proposal was accepted, and the first number of the book appeared in September 1848.

At that time Mr. Grant was not connected with this Society, and it was not until the year following that, through the kind offices of Mr. Woodfall, of the well-known house of George Woodfall and Co., printers, he obtained an introduction to your Secretary, Admiral Manners, an event which he remembers with gratitude, and speaks of as having made his course smooth. Until that time his access to books of reference of very recent date had been often troublesome and precarious. The History was finally completed, and published in the spring of 1852.

It would be quite out of the question on this occasion to attempt a detailed criticism of a work like that before us, and yet you have a right to expect from me such a sketch of its contents as may enable you to form some judgment on the propriety of your Council's award. This I will proceed to give as briefly as I can.

The first thirteen chapters of the book are devoted to an Historical Exposition of the Theory of Gravitation, from Newton's first conceptions, through the gradual developments it has undergone at the hands of Euler, Clairaut, D'Alembert, Lagrange, Laplace, Poisson, Plana, Airy, Lubbock, Hansen, and others, till the last great achievement of Adams and Le Verrier. This inquiry forms by far the most laborious portion of the volume. To collect his materials, the author had not only to wade through the pages of a multitude of special treatises, but also to search the published records of all the great Academies of Europe. Then the arrangement, in anything like lucid order, of the vast mass which he had accumulated in the narrow compass of an octavo volume, was no slight difficulty; and if we further consider that his facts were to be stated in language which was to satisfy the mathematician, and to be intelligible to the educated public, I think it admits of question whether the task of construction was not as great as that of collection and discussion.

In this part of the book he carries us through the theory of planetary perturbations as gradually developed by Euler, Lagrange, Laplace, and Poisson, showing at each step how much each accomplished, and where each failed. He gives a complete account of the researches of Lagrange and Laplace which demonstrated the stability of the planetary system; of the acceleration of the moon's mean motion, from its detection by Halley to the discovery of its cause by Laplace; of the theory of *Jupiter's* satellites; and of the discovery of the planet *Neptune*.

This, though the most laborious, does not constitute the largest portion of the volume. All that is known of the phy-

sical constitution of sun, planets, and comets, is given in great detail in the fourteenth and fifteenth chapters, together with many valuable contributions to the literature of those subjects. Nor has the author omitted to trace the history of observational astronomy from the earliest period to the present time; or to acquaint his readers with those grand views touching the structure of the heavens, of which Wright, of Durham, appears to have been the original propounder, and to which the genius of William Herschel and William Struve has since given importance.

This, Gentlemen, is a very rapid, and I confess a very inadequate statement of the contents of a book, of which the best praise is, that it has satisfied those who have had occasion to refer to it. For myself, I do not profess to have examined it rigorously, though of course during the last few weeks it has been my duty to look through it very carefully. I confess to have been astonished at the clearness and the completeness with which in so small a compass he has conveyed so much information. There is only one point where he appears, in my opinion, to have fallen short;— he has not given sufficient prominence to the labours of the illustrious author of *Theoria Motûs Corporum Cælestium* — an oversight occasioned, as I afterwards found, by a difficulty in arrangement which could not be easily obviated.

Throughout the book no one can fail to be struck with the rare skill, integrity, and discernment, the author has displayed in tracing the successive stages of progress; or with the scrupulous care he has taken to assign to each of the great men whom he reviews, their proper share in the common labour. Nowhere is this more conspicuous than in the discussion relative to the discovery of the planet *Neptune*. By a simple narration of facts, he has placed the history of that great event in so clear and so true a light that I believe I am not wrong in saying, he has gained an author's highest praise under such circumstances — the approval of both the eminent persons concerned.

The same spirit of candour and fairness pervades every page. Everywhere you will find that delicacy and consideration for other men's reputations, which are characteristic of a liberal mind. I will assert without fear of contradiction, that there is not a flippant or inconsiderate passage—not *one* calculated to wound unjustly the memory of the dead, or the feelings of the living; no offensive nationality seeking to pervert truth under the garb of patriotism.

At the same time, with all this disposition to render deference and respect, you will find no connivance with undue assumption.

Thus he vindicates for Euler the first conception of the theory of the variation of arbitrary constants: frequently ascribed to Lagrange.

He claims for Picard, against Dominique Cassini, the discovery of the effect of temperature on atmospheric refraction.

He has justly assigned to Tycho Brahé and Thaddeus Hagech (better known as Hagecius) the invention of the method for

determining the place of a celestial body by meridian observations, —an invention which has been referred on high authority to the astronomers of the seventeenth century.

He shows that the Method of Equal Altitudes, which has been attributed to Picard, is really due to Thomas Digges, who published it as early as 1573.

He defends Roemer, the inventor of the Transit Instrument, against a most unjust insinuation, tending to deprive him of the merit of that invention.

I could cite other passages of the same kind, but these are enough to show how deep and independent have been our author's researches.

It is, then, for this book, as a great and important contribution to our literature, solely on its own intrinsic merits, irrespective of every consideration of the circumstances under which it was produced, that your Council has resolved to award to Mr. Robert Grant your Gold Medal, as the highest tribute they can pay to his indefatigable zeal and talent, and of appreciation for the service he has rendered. I hope this act of your Council will meet with your approval.

The President then delivering the Medal to Mr. Grant, addressed him in the following terms:—

Mr. Grant,—In the name of the Royal Astronomical Society I have the honour to present to you this Medal, as the highest tribute it can pay to the ability, the learning, and the industry you have displayed in your work entitled *A History of Physical Astronomy*, and as an acknowledgment of the great service you have thereby rendered to our science.

I am sure every Fellow of this Society will unite with me in warmest wishes that your health may be long spared to reap fresh honour in the great field of research which you have chosen, accompanied by more substantial rewards than it is in our power to confer.

The Society is probably not aware of some of the circumstances of your early life, and I have purposely abstained from mentioning them in my address, lest I should seem to wish, by such means, to influence their judgment on the resolution of the Council; otherwise, Sir, I might have dwelt on the dispensation of Providence which incapacitated you, in the bloom of promising boyhood, for exerting those faculties which you most value. I could have said, that during those six precious years of life—from fourteen to twenty—when most men are preparing for the active duties of life, you were laid on a bed of sickness—that with the unexpected return of bodily strength, the mind, too, began to yearn for nourishment —that you became your own instructor, and, with no other help than books, supplied by the affectionate care of your relatives, you qualified yourself for the high task you have now so successfully accomplished. I could have said more, but these, Sir,

are incidents which, though they enhance our interest in your book and our respect for you, did not, as I have said, influence the members of the Council, to most of whom, I believe, the incidents were unknown. They certainly have not influenced the Society to-day. This award is a deliberate expression of the high opinion we entertain of the value of your book as a contribution to the scientific literature of your country, and as such we beg you will accept it.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President:

MANUEL J. JOHNSON, Esq., M.A. Radcliffe Observer.

Vice-Presidents:

G. B. AIRY, Esq. M.A. F.R.S. Astronomer Royal.
AUGUSTUS DE MORGAN, Esq.
Rev. ROBERT MAIN, M.A.
Rev. BADEN POWELL, M.A. F.R.S.

Treasurer:

GEORGE BISHOP, Esq. F.R.S.

Secretaries:

WARREN DE LA RUE, Esq. F.R.S.
Admiral R. H. MANNERS.

Foreign Secretary:

JOHN RUSSELL HIND, Esq. Superintendent of the
     Nautical Almanac.

Council:

RICHARD C. CARRINGTON, Esq.
Rev. GEORGE FISHER, M.A. F.R.S.
JAMES GLAISHER, Esq. F.R.S.
ROBERT GRANT, Esq. M.A.
JOHN LEE, Esq. LL.D. F.R.S.
Rev. C. PRITCHARD, F.R.S.
WILLIAM RUTHERFORD, Esq. LL.D.
WILLIAM SIMMS, Esq. F.R.S.
Admiral W. H. SMYTH, K.S.F. D.C.L. F.R.S.
S. C. WHITBREAD, Esq., F.R.S.

ROYAL ASTRONOMICAL SOCIETY.

MANUEL J. JOHNSON, Esq. President, in the Chair.

Hale Wortham, Esq., Royston, Herts ; and
Thomas Minchin Goodeve, Esq., King's College,
were balloted for and duly elected Fellows of the Society.

In the List of Fellows of the Society recently published, the
date of Lord Wrottesley's election is stated to be 1829, February
29. This is a mistake. The true date is 1820, February 29,
his lordship having been one of the founders of the Society.

Information having been received from time to time, especially
from Associates of the Society residing on the Continent, respecting
irregularities in the transmission of the *Monthly Notices* to their
respective destinations, it has been resolved, in future, to despatch
the successive numbers, in all possible cases, by post, immediately
after publication, the Society defraying the expense of postage.
It is to be hoped that by this arrangement the irregularities which
have occasioned so much annoyance will be effectually removed ;
but should a repetition of them occur in any instance, it will be
desirable to give due notice thereof to the Secretaries of the
Society.

The publication of the forty-third volume of the *Astronomische
Nachrichten* has recently been commenced. Fellows of the Society
desiring to become subscribers are requested to send an intimation
to that effect to Mr. Williams, the Assistant-Secretary of the
Society, who will also receive the subscriptions, which are *payable
in advance.* The price of each volume is fifteen shillings. There
generally appear two volumes in a year. The successive numbers
of the Journal are delivered in England free of any charge for
postage.

Discovery of a New Planet.   By M. Chacornac.

Admiral Smyth has received a letter from M. Chacornac, announcing the discovery of a new planet by him on the 8th of February, at the Imperial Observatory, Paris. The following observed positions of the new body have also been forwarded by M. Chacornac. The star of comparison is 21963 Lalande :—

	Paris M.T. h m s	δ R.A. m s	δ Decl. ′ ″	No. of Comps.
Feb. 8, 1856	14 4 3·44	5 6·29	......	3
,, ,,	14 27 35·44	......	1 49·97	3
,, ,,	15 33 40·54	5 8·23	......	2
,, ,,	16 36 36·34	......	1 14·50	2
Feb. 9 ,,	12 30 10·39	5 35·82	......	5
,, ,,	13 7 53·51	......	4 17·35	3

Mean position of the star of comparison for January 1856, deduced from the Catalogue of Lalande :—

R.A.   ... $11^h\ 26^m\ 58^s·03$

Decl.   ... $4^\circ\ 55'\ 6''·4$

Elements of Leda.   By M. C. F. Pape, Assistant at the Altona Observatory.

(*Communicated by Professor Challis.*)

M	....	20 10 13·1 1856, Jan. 13·45294 Berlin M.T.
π	....	91 33 51·3 } M. Eq. 1856·0
☊	....	296 2 26·2
i	....	6 31 19·7
φ	....	11 0 50·4
Log a	......	0·438739
Log μ	......	2·891899

These elements have been computed from the Paris observation, Jan. 13; one at Bilk, Jan. 20; and one at Berlin, Jan. 24.

Note on Saturn's Rings.   By Warren De La Rue, Esq.

(*Extract of a Letter to the Editor.*)

" I have observed some phenomena in reference to *Saturn's* Rings, which I think it desirable to bring under the notice of

astronomers, as likely, if corroborated, to throw much light on the physical condition of the planet. For the last five or six years that I have been assiduously observing, measuring, and making drawings of *Saturn*, I have noticed irregularities in the widths of especially the two bright rings, and in the dark division between them, only to be accounted for, as it appears to me, by supposing, 1st. That the centre of the rings is not coincident with the centre of the planet; 2d. That the rings are not of the same breadth throughout; 3d. That they are, as proved by the form of the shadows, situated in different planes, and, moreover, that the nodes of the rings have a somewhat rapid motion. For I have frequently observed the rings wider at one extremity of their minor axes than accords with their breadths at the extremities of the major axes. Also, I have observed the same phenomena with respect to the principal dark division ; and, moreover, that sometimes the eastern ansa of one of the bright rings is wider than the western, and *vice versâ*, by quantities quite appreciable to a practised eye, although difficult to determine exactly by measurement. From the concave form of the shadow, as at present seen on the middle ring, it is evident that this ring is elliptical in its section, and, I believe, of considerably greater thickness than that assigned usually to it. It would be very desirable to have the depth of this curvature of the shadow well determined by accurate micrometrical measurements, as it would afford data for determining the form and thickness of at least the middle ring.

" 7 *St. Mary's Road, Canonbury, February* 2, 1856."

Note on the Rings of Saturn and on the Orbit of α Centauri. By Captain W. S. Jacob.

(Extract of a Letter to the Editor.)

" We arrived here on the 29th December, and the accompanying drawing will show you that I have not been long in recommencing work. It represents *Saturn* as I saw him on the 8th instant at $6\frac{1}{2}^{h}$. The concave outline of the shadow I see plainly enough, yet cannot but think it an illusion; the more so as in a subsequent careful scrutiny, on the 10th, I could not be *quite sure* that the outline was not a straight line. The edge of the shadow in the faint ring I am unable to see, only I cannot trace the ring up to the planet in that part. I am quite unable to imagine any section for the bright ring (compatible with its ascertained thinness) that would give such an outline to the shadow. I am making a series of micrometric measures of the rings, and will communicate the result when complete.

" α *Centauri* has advanced considerably, and come to very

nearly its minimum distance for the present; five days' observations give:—

<div align="center">

Epoch, 1856·015   302°·24   3″·806

</div>

The periastral time will not, I think, differ much from 1862·5.
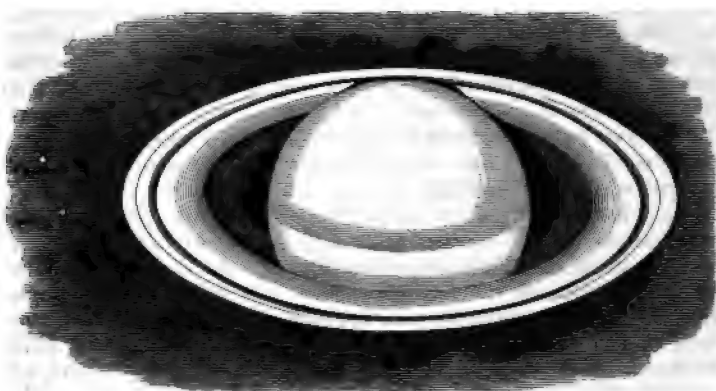
"Madras, January 12, 1856."

Note on Solar Refraction.   By Professor C. Piazzi Smyth, Royal Observatory, Edinburgh.

This term of "solar refraction" was given by Professor W. Thomson to characterise an effect which he had deduced theoretically from the dynamical theory of heat, and, if proved to exist, is pregnant with important consequences to every part of astronomy.

For it at once infers the necessity of the existence of a medium pervading space,—a medium, though rare, of similar constitution to our own atmosphere, and undergoing by necessity a condensation in the neighbourhood of the sun. Hence, he showed, that there cannot but arise a refraction of objects beyond the sun, when this body crosses their line of direction.

The theory could do but little beyond pointing to the fact of some amount of such solar refraction, while the exact amount could only be ascertained by astronomical measures. But with a comparatively small number of such observations, there seemed thus a promise of obtaining speedily a quantitative result,—a result, too, bearing immediately on the much-vexed question of a resisting medium, to approach which, at present, astronomers have scarcely any other method than that of cometary perturbations, wherein are mixed up so many other unknown quantities, and wherein the opportunities for observation are so rare, that generations may pass away before anything decisive is arrived at.

I determined, therefore, to inquire into the subject from Professor W. Thomson's point of view; and the best practical method at my command for testing this "solar refraction" seemed to be the observation of stars transiting the meridian in the neighbourhood of the sun; and for this purpose the large object-glass of the Edinburgh transit instrument was very favourable.

But although extraordinary precautions were taken in darkening the observing-room, and using various devices to improve the telescopic vision, it was found that the thick atmosphere of a town, and one so nearly on the sea-level, was almost always so brilliantly illuminated in the neighbourhood of the sun, that no stars could be observed, or even seen, under the desired conditions. On one occasion, however, in the past history of the Observatory, a unique state of the air enabled a star to be observed in what might be expected to be a possible refracting distance from the sun, while two others had also been observed the same day, at a distance so much greater, that they might safely, as a first approximation, be considered to be out of the range of disturbance.

Now, according to Professor W. Thomson's deduction from the dynamical theory of heat, the star in the neighbourhood of the sun, *α Orionis*, should have appeared closer to the other stars, *β Orionis* and *α Aurigæ*, on that day than at any other time of the year; and there were plenty of observations of the same stars during other months, when they all transited at night, completely out of reach of the solar influence.

What result, then, do the observations show? Why, after special computation, which has not sensibly altered the result from the original computation, made before the appearance of the dynamical theory of heat, *α Orionis* appears to have been visibly closer in R.A. to *β Orionis* by 0·06 of a second of time; and closer to *α Aurigæ* by 0·04 of a second of time.

The two results are, therefore, confirmatory of each other, and of the existence of the "solar refraction," and with that of a resisting medium filling space, and forming a material connexion still, and strengthening the idea of unity between the sun and all the planets.

But can we depend on this result? Or ought we to be satisfied with it? The mere arithmetic of it looks well; but those who have had much practice in striving after the highest attainable exactness, and know the innumerable sources of possible error in every astronomical operation, would very properly not be content when the effect sought for is so excessively minute, except with a large number of observations, and on many different stars; some, too, with the sun seen between them, and in the direction of N.P.D. as well as of R.A.

How, then, are such observations to be obtained? As far as my experience goes, there is no chance of obtaining them with any instruments at any observatory already established; and this by reason of the great depth of illuminated atmosphere through which such observatory must always look. But if our instruments could

be temporarily transported to the summit of such a mountain
as the Peak of Teneriffe, where 10,000 feet in depth of the grosser
part of the atmosphere would be eliminated, there is every proba-
bility that a satisfactory result would be obtained in the course
of a single summer.

Not only, too, would a knowledge of a most important element
in the constitution of the solar system be procured, but we should
have a proof of the cosmical character and universal bearing of
the dynamical theory of heat,—a theory which has been elaborated
by the mathematicians of our own time and our own country.

Observations hereinbefore referred to.

Date, 23^h, June 20, 1838.

Name of Star.	Distance from Sun in R.A.	Distance from Sun in N.P.D.	Transits (5 Wires) Corrected for Error of Coll. Level and Azimuth.	Tabular Apparent Places.
	h m	° ′	h m s	h m s
α Aurigæ	0 54	+22 22	5 3 58·91	5 4 43·70
β Orionis	0 52	+31 52	5 6 0·37	5 6 45·14
α Orionis	0 12	−16 6	5 45 39·29	5 46 24·12

From these numbers flow the following results :—

Names of Stars Employed.	Difference Observed.	Difference Computed.	Difference of Obs^d and Comp^d, or Solar Refraction
	m s	m s	s
α Orionis − β Orionis	39 38·92	39 38·98	+0·06
α Orionis − α Aurigæ	41 40·38	41 40·42	+0·04

In re-computing these observations for the special purpose now
in view, I have not found any reason for altering the corrections
for errors of collimation level and azimuth adopted at the time by
Professor Henderson. Some sensible difference came out between
our clock *errors*, but none between our *rates;* and this rate, which
was alone of importance in the new inquiry, was shown to be under
0·01 of a second.

This resulting inappreciable effect of the clock-rate is, too, all
the more satisfactory, inasmuch as I computed the corrections to
the *apparent* places of the stars with new constants adapted to the
instant of observation, and derived the *mean* places from all the
Edinburgh measures made in the year in question.

As regards, then, the possible inaccuracy of the numerical
corrections for error of instrument and clock, the upper limit must,
I think, be considered to be less than the fraction representing
the expected solar influence. But there is still the question of the
sufficiency or power of accuracy of the original transits observed,
especially seeing that they are each observed over five wires only.

To enable astronomers to form their own opinion on this point,
I submit herewith the differences of each wire observed, from the

time computed for it from the mean of the whole number of wires, the measured value of the intervals and the declination of the star.

Error of Observation of each Wire on the Mean of the Five.

Name of Star.	1st Wire.	2d Wire.	3d Wire.	4th Wire.	5th Wire.
	s.	s.	s.	s.	s.
α Aurigæ	+0·102	+0·058	−0·003	−0·109	−0·049
β Orionis	+0·032	+0·053	−0·036	+0·013	−0·062
α Orionis	+0·058	+0·046	+0·024	+0·041	−0·168

There may be some difference of opinion as to what the probable error of the mean for each star may be, but there can be little doubt of its being under the now declared quantity of "solar refraction;" and there can be no doubt at all as to the merit of the observer, Mr. Alexander Wallace, the assistant astronomer of the Observatory, the characteristic excellence of whose transits for many years past has enabled the three observations now under discussion to assume an importance which has seldom fallen to the lot of any other three transits.

Royal Observatory, Edinburgh, Jan. 9, 1856.

Note on the Orbit of α Centauri and on the Rings of Saturn. By Captain W. S. Jacob.

(*Extract of a Letter to the Editor.*)

" I have to communicate a matter of great interest regarding α *Centauri*. Finding from my observations communicated by last mail that the pair must have come to about their minimum of distance, I thought something like a good approximation to the orbit might be procured, especially as the observations of Richaud in 1690, and Feuillée in 1709, seem to bring both the period and perihelion passage within very narrow limits :* viz. the former between 77^{yrs} and 79$^{yrs·}$5, and the latter between 1862·4 and 1844·2. But on laying down an ellipse which would pass through the positions of 1834, 1848, and 1856, and computing intermediate points, to my dismay I found *enormous* errors, and the largest of all· at those epochs which had been best observed, and where the observations were most accordant *inter se*, viz. about 1852–3. I then set to work to project the observed distances as well as angles into a curve, with the time for a co-ordinate, and on attempting to bring these into agreement, found them altogether incompatible, not only with each other, but with any kind of elliptic motion. Lastly, I took out the places independently from the two curves without

* These points will be more fully discussed in a paper about to be presented to the Society by E. B. Powell, Esq.

any sort of adjustment, merely reading off the angles and distances for each complete year, and laid them down in contiguity with the ellipse; the result is shown in fig. 1, where these places are marked +, and exhibit a very regular epicyclic curve, in which the revolution of the star round the proper elliptic place can be traced throughout, the places corresponding to Kepler's law of equal areas being marked by the short oblique. lines cutting the curve.

"In order to show how very little the actual observations are altered by projecting them into curves, I have laid down in fig. 2 on a larger scale the places* as taken from the observation books, *without any preparation* beyond taking the means for the different epochs; and although, as might be expected, the curve is not quite so regular as before, yet its course is marked with sufficient distinctness. (The places are marked o.) The jump from *a* to *b* is partly accounted for by a change from daylight to night observations—the day measures of distance being usually shorter than those by night. I think, then, there can be no hesitation in pronouncing on the existence of a disturbing body. The disturbance is much more strongly marked than in the case of 70 *Ophiuchi*,† where it was the result of calculation and of a balance of errors; whereas here it is visible to the eye by inspection of the *unprepared observations*. Of course I am wrong in making the observed and mean places coincide at 1856·0, but there is no help for this, as the period of the disturbance is unknown, and will continue so for some years, as we have at present only six years of really accurate observations. The errors introduced by this assumption will not be very important, as they will be spread gradually over all the preceding places.

"Supposing my view of the matter to be correct, the distance must now be on the increase, and the place of 1857·0 cannot differ much from

$$317°·5 \qquad\qquad 3''·90 +$$

"The following are the results of five nights' measures of *Saturn* and his rings reduced to his mean distance; epoch, 1856·04442, Greenwich mean time :—

Major Axis of Outer Ring, outside	39·997
— ' — fine line	37·966
— — inside	35·820
— Inner Ring, outside	34·859
— — inside	26·271
— Obscure Ring	22·214
— Planet	17·940
Minor Axis of Outer Ring	18·754

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* From 1850 to 1856 being the only period in which the distance measures can be depended on.

† *Vide Monthly Notices*, vol. xv. p. 228.
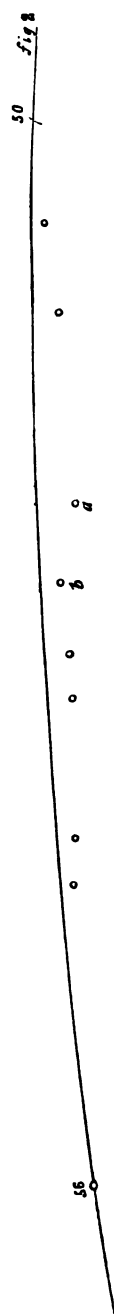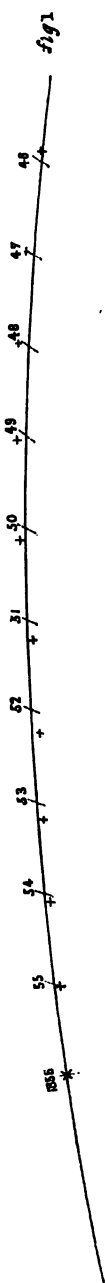
" These agree very closely with the measures of 1853, the chief difference being in the outer ring *inside*, which, as you may see in my engraving, has not, to my eye, a definite boundary.

" With reference to my drawing of *Saturn*, sent by last mail, I have since, on the 22d inst., seen under good definition, with power 365, the shadow on the ring *convex* throughout, but with a kind of projecting *ear* just where it crosses the dark space between the rings, which, when seen less distinctly, or with a lower power, is doubtless what has given rise to the impression of concavity; as it was, the phenomenon was visible only by glimpses when the atmosphere was at its best, and the focus very exactly adjusted. The same thing was seen afterwards with a Huyghenian eye-piece of about 300. It was somewhat as represented in the accompanying sketch; * the exact form of the projection could not be made out, but it had rather a triangular appearance, and was seen distinctly crossing the dark division, which was much paler than the shadow; the one indeed was *black* and the other brown; the division, therefore, is not mere empty space, but filled with matter of some kind, since a shadow can be seen upon it, and from the form of the shadow it would seem to be depressed below the level of the other rings, though I confess myself unable to suggest the kind of section it must have to account for so peculiar a form of shadow.

" P.S. The shadow could also be distinctly seen upon the obscure ring.

" *Madras, January* 19."

Apparent Right Ascensions and North Polar Distances of recently discovered Small Planets, observed at the Royal Observatory, Greenwich, 1856, *January and February.*

The observations of N.P.D. are corrected for Refraction and Parallax.

Urania.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1856, Jan. 2 14 14 26·1	9 2 3·20	72 36 8·56
30 11 56 56·1	8 34 34·28	71 15 26·36
31 11 51 55·7	8 33 29·56	71 12 31·76
Feb. 14 10 43 14·1	8 19 48·47	70 38 39·46

* This was exhibited at the meeting of the Society.— ED.

Pomona.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1856, Jan. 24	13 50 54·9	10 5 12·50	87 24 24·02
30	13 23 35·0	10 1 27·39	87 11 25·61
Feb. 12	12 21 46·0	9 50 43·45	86 20 12·44
14	12 12 8·0	9 48 57·00	86 10 6·08

Jan. 24. There is some doubt whether the above object is the planet.

Parthenope.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1856, Jan. 10	7 31 42·8	2 49 46·21	79 5 52·37

Leda.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1856, Jan. 28	11 54 25·9	8 24 10·58	72 42 31·59
29	11 49 30·7	8 23 11·12	72 42 37·06
Feb. 12	10 41 57·7	8 10 38·78	72 47 5·72

Victoria.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1856, Jan. 15	14 48 53·4	10 27 51·42	92 50 2·50
24	14 7 37·8	10 21 58·08	....
30	13 39 43·8	10 17 38·88	92 47 21·30

It appears probable that the object observed on Jan. 15 is not the planet.

Psyche.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1856, Jan. 15	8 3 6·8	3 40 58·24	74 24 3·78
23	7 33 32·7	3 42 51·74	74 4 53·28

Euphrosyne.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, Jan. 30 14 37 5·3	11 15 9·81	42 59 44·45
Feb. 14 13 21 47·1	10 58 47·59	41 56 17·04

Iris.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, Jan. 11 10 6 59·0	5 29 24·52	69 37 32·40
15 9 49 25·3	5 27 34·13	69 49 26·05
25 9 8 41·9	5 26 9·61	70 10 4·46
28 8 56 30·6	5 25 46·00	70 17 20·98
29 8 52 41·9	5 25 53·29	70 18 47·72
30 8 48 55·2	5 26 2·52	70 20 15·99
Feb. 16 7 49 54·9	5 33 53·90	70 32 52·15

Polyhymnia.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, Jan. 15 13 23 5·2	9 1 49·19	70 29 26·92
Feb. 12 11 8 3·0	8 36 48 41	....

Egeria.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, Jan. 30 14 34 35·7	11 12 39·79	56 11 49·65
Feb. 14 13 23 40·5	11 0 41·29	54 30 23·23

The object observed on Jan. 30 is probably not the planet.

Description of an Observatory erected by Frederick Brodie, F.R.A.S., at Eastbourne, Sussex.

The following short description of what may be termed a *portable* observatory may be useful to those amateur astronomers who may not be permanently resident at any one place for many

years together, or who may possibly change their residence several
times during the course of their lives. In proof of the feasibility
of this plan, the author first erected this observatory in Somerset-
shire, and has since removed it into Sussex, some 160 miles distant.
The time occupied in taking the whole of it to pieces, packing
up the parts in sequences of numbers, and placing it on a railway,
was from *four* to *five days*. The re-erection of the building will
take at least ten days or a fortnight.

The building is entirely of wood, and rests on about 20 small
brick pillars, which rise to about 8 or 10 inches above the ground,
so as to keep the building free from damp. Upon these small
pillars rests the *bottom cill*, 5 in. by 4 in., from which rise uprights,
2½ in. by 3½ in. at about intervals of 2 feet apart. Those uprights
which support the dome are 4 in. by 4 in., and are placed only at
the angles of the dome-room. The *top cill* is 4 in. by 3 in., upon
which rests a flat roof of ¾-in. boards, having a small rise of 3 in.
in the centre, to throw off the wet, and covered with canvass well
painted with paint and varnish. The top cill of the dome-room is
5 in. by 4 in., upon which is placed a cast-iron ring in segments,
slightly grooved for the balls of the dome to run upon. This top
cill is circular, 15 feet diameter inside. The dome-room is a
figure of 12 sides, two of the sides forming a connexion with the
transit-room. It has also two windows.

The dimensions of the building are as follows:—

			ft.	in.
Diameter of dome at bottom	...	...	15	0
Ditto ditto at top	...	...	6	6
Height of dome	...	...	6	6
From floor to top of dome	...	...	14	6
Length of transit-room ...	...	...	20	6
Width of ditto	...	...	8	0
Height of ditto from floor	...	...	7	3

A portion of the transit-room, about 7 feet in length, is par-
titioned off, forming an entrance lobby, &c. The floor of the
transit-room is about 3 in. above the bottom cill, and that of the
dome-room about 4 in. above the transit-room.

The whole building is weatherboarded with 1-in. boards, all
screwed on; the floors also are screwed down; nothing is nailed
except fixtures, which do not require taking to pieces. The
uprights and cills are morticed together and fixed with wooden
pegs, so as to admit of being driven out when required. The
weatherboarding has a lap of 1 in., upon which lap is tacked a strip
of list, which keeps the sides weather-tight when screwed up. The
floors and roof of transit-room are tongued with ¾-in. iron hooping.

The transit-shutters are 15 in. wide in clear opening. The
top shutter is opened by a rope and pulley. The north shutter has
a lens fixed in it of about 50 feet focus, at which distance a *meri-
dian mark* is placed. This mark consists of a brass pin, ¾ in.

diameter, one end of which plays into two parts, for convenience of imbedding in brickwork; upon the pin fits a disc of brass 2½ in. diameter, and turning freely upon the pin, having a small wedge, to fix it, running through its centre. On one side of this disc is a small disc of iridium metal, having a black dot in its centre, in the centre of which, again, is a fine point of the iridium metal, having the effect of a white speck on a black ground. This metal is preferable to silver, it having no tendency to oxidise in the open air.

The lens which is fixed in the shutter of the transit, having a focus of 50 feet, at which distance the meridian-mark is placed, is only useful for collimating the transit-wires, or other temporary purposes, while adjusting the instrument; because the mere fact of its being attached to a wooden cell, which is ever liable to warp, and thereby slightly change the position of the lens, renders it totally unfit for a permanent reference to the meridian mark, since any such alteration in the position of the lens will disturb the coincidence of the transit-wires and meridian-mark, making the appearance of an alteration in position of the meridian-mark itself, or the transit-instrument.

The *transit-instrument* is a 3-in. glass, by Merz, of Munich, 45 in. focal length, and rests on a cast-iron stand, fixed upon brickwork, having one circle on its axis, 10 in. diameter, divided to 15 minutes, and reading to 15 seconds. It has a sliding eye-piece, worked by a double-threaded screw, with a diagonal eye-piece for observing stars in zenith.

The *clock* is fixed to a post formed of two 3-in. planks, bolted together in the form of a T, and is fixed in rubble-work sunk in the ground. A Hardy's noddy on the top shows no tremor at any time.

The *dome* is in the form of a 12-sided cone without the apex. It is 15 feet diameter at the bottom, tapering to 6 ft. 6 in. at the top, having a height of 6 ft. 6 in. It has a sliding-shutter on either side, the clear opening of which is 21 in.   These shutters are fitted with small brass runners, and work in grooves lined with thick hoop-iron, causing the shutter to slide freely.   The top shutter on the dome is hung on hinges, and opens with a rope and pulley. The flat top of dome is covered with canvass well painted. The bottom cill is made of segments of wood 5 in. by 2 in. in two layers, put together with marine glue, and bolted with 4-in. coach-screws, the segments breaking joint with each other all round. There are four places where the joint is left unglued for the purpose of taking it to pieces. On the under-side of this cill are screwed two rings of bar-iron, 1 in. by ½ in. thick.   These rings have an interval of 2 in. between them, so as to catch the cast-iron balls upon which the dome turns, on the right-angled corner of the bar-iron. By this arrangement a groove is formed for the balls, which has the effect of keeping the dome quite steady; and so easily is it turned, that even a single finger is sufficient for that purpose. An iron bar forms a handle by which the dome is turned.   Although

so light and so easily moved, the form of the dome, and the manner in which it is hung, are such, that there is hardly any motion perceptible during the heavy south-west gales so prevalent on this coast. The top cill of the dome is a 12-sided figure made of wood-scantling, 2½ in. by 3½ in.; to this the rafters are screwed. The rafters are 2 in. by 3 in. An elm ring of board, ½ in. thick and 6 in. broad, is screwed on outside the lower cill of dome, to give it stiffness, and to keep out rain and wind.

The equatorial telescope is made by Merz, of Munich; the object-glass is 6·4 in. in diameter, having a focal length of 8 ft. 6 in. The tube is made of wood, and is very light. The mounting is of peculiar construction, made in England, from drawings and designs of the author. It is a stand of cast-iron, in three pieces, each bolted firmly together, having lead between the joints, to lessen any liability to tremor. The bottom part is entirely below the floor of the observatory, and is a triangular plate, from which rises the upper part of the stand to a height of 6 feet. The northern side of the stand tapers from 3 ft. 6 in. at the bottom, to a width of 6 in. at the top. The adjusting screws for latitude and azimuth are attached to the bottom part of the frame. The polar axis is also bolted to the stand at the upper part, having two taper bearings bushed with brass; diameter of the bottom one, 3¼ in.; top one, 2½ in. A cast-iron cradle turns on this axis, the bearings of which are also bushed with brass. To this is attached the hour-circle, while the top of this cradle carries the declination axis. On the top of the polar axis, and inside, at the top of the cradle, are two corresponding discs of steel, upon which the whole weight of the telescope cradle and declination axis rests; so that the cradle turns with a very small amount of friction. There is an adjusting screw, which acts on the discs, so as to lift the cradle from the taper bearings just enough to allow it to turn freely without lateral motion. The declination axis is made of brass, and is hollow; the end carrying the cradle of telescope has a bearing of 2½ in. diameter; the other end has a bearing 2 in. diameter, and carries the circle and counterbalance. The declination circle is 18 in. diameter, and is divided on silver to 10″ reading to 10″ by the verniers. The hour-circle is 15 in. diameter, divided on silver to 2′ reading to 4″ by vernier. On the polar axis is fixed the clockwork and tangent screw. There is a shifting counterbalance fixed on the telescope for using with the position micrometer. The weight of the stand is about three-quarters of a ton, and the weight of the moving parts, together with a light tube, renders the telescope remarkably steady, and perfectly free from oscillation of any kind. The bottom part of the stand rests on three brick piers laid in cement.

To any gentleman who might wish for further information, the author will be happy to afford it.*

February 8, 1856.

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* This description was accompanied with two photographs, which were exhibited at the meeting of the Society.—Ed.

The Minor Planets.

The following Table of the Minor Planets has been drawn up by Mr. Pogson of the Radcliffe Observatory, Oxford:—

No.	Name.	First Discoverer.	Place and Date of Discovery.		Period in days.	Ascen. Node.	Inclin- ation.
						°	°
1	Ceres	Piazzi......	Palermo ...	1801, Jan. 1	1681	81	11
2	Pallas	Olbers	Bremen	1802, Mar. 28	1683	173	35
3	Juno	Harding....	Lilienthal ..	1804, Sep. 1	1592	171	13
4	Vesta	Olbers	Bremen	1807, Mar. 29	1325	103	7
5	Astrea	Hencke	Driesen	1845, Dec. 8	1510	142	5
6	Hebe	Hencke	Driesen ····	1847, July 1	1379	139	15
7	Iris	Hind	London ····	1847, Aug. 13	1347	260	5
8	Flora	Hind	London ····	1847, Oct. 18	1193	110	6
9	Metis	Graham	Sligo........	1848, April 25	1346	69	6
10	Hygeia	De Gasparis.	Naples.....	1849, April 12	2041	288	4
11	Parthenope .	De Gasparis.	Naples.....	1850, May 11	1402	125	5
12	Victoria....	Hind	London	1850, Sep. 13	1303	235	8
13	Egeria	De Gasparis.	Naples.....	1850, Nov. 2	1511	43	17
14	Irene	Hind	London	1851, May 19	1518	87	9
15	Eunomia ...	De Gasparis.	Naples.....	1851, July 29	1570	294	12
16	Psyche.....	De Gasparis.	Naples.....	1852, Mar. 17	1825	151	3
17	Thetis	Luther.....	Bilk	1852, April 17	1420	125	6
18	Melpomene .	Hind	London	1852, June 24	1270	150	10
19	Fortuna....	Hind	London	1852, Aug. 22	1395	211	2
20	Massilia....	De Gasparis.	Naples.....	1852, Sep. 19	1366	207	1
21	Lutetia	Goldschmidt.	Paris	1852, Nov. 15	1387	80	3
22	Calliope....	Hind	London	1852, Nov. 16	1809	67	14
23	Thalia	Hind	London	1852, Dec. 15	1554	68	10
24	Themis	De Gasparis.	Naples.....	1853, April 5	2033	36	1
25	Phocea	Chacornac..	Paris	1853, April 7	1359	214	22
26	Proserpina..	Luther.....	Bilk	1853, May 5	1580	46	4
27	Euterpe....	Hind	London	1853, Nov. 8	1313	94	2
28	Bellona	Luther.....	Bilk	1854, March 1	1689	144	9
29	Amphitrite..	Marth	London	1854, March 1	1491	356	6
30	Urania.....	Hind	London	1854, July 22	1329	308	2
31	Euphrosyne.	Ferguson...	Washington.	1854, Sep. 1	2048	31	26
32	Pomona....	Goldschmidt.	Paris	1854, Oct. 26	1517	221	5
33	Polyhymnia.	Chacornac..	Paris	1854, Oct. 28	1772	9	2
34	Circe	Chacornac..	Paris	1855, April 6	1591	184	5
35	Leucothea ..	Luther	Bilk	1855, April 19	1800	357	8
36	Atalanta....	Goldschmidt.	Paris	1855, Oct. 5	1685	359	19
37	Fides	Luther.....	Bilk	1855, Oct. 5	1456	8	4
38	Leda	Chacornac ..	Paris	1856, Jan. 12	1662	296	7
39		Chacornac ..	Paris	1856, Feb. 8			

Positions Moyennes pour l'Epoque de 1790·0 *des Etoiles Circum-*
polaires, dont les Observations ont été publiées par Jérome
Lalande dans les Mémoires de l'Académie de Paris de 1789 *et*
1790. Par Ivan Fedorenko, Astronome Surnuméraire à l'Ob-
servatoire de Poulkowa. St. Petersbourg, 1854.

It is generally known that MM. Hansen and Nissen calcu-
lated tables to facilitate the reduction of the stars contained in the
Histoire Céleste of Lalande, which were published in 1825 by
Schumacher, and that these tables were employed in the final
reduction of the same stars executed under the auspices of the
British Association, the results of which were published in 1847.
But, independently of the observations of the *Histoire Céleste*,
the *Memoirs of the Academy of Sciences* for 1789 and 1790 contain a
considerable number of similar observations, for which auxiliary
tables had not been calculated, and which, consequently, were not
included in the catalogue published by the British Association in
1847. The author, accordingly, at the suggestion of M. W. Struve,
undertook the reduction of these observations. The results are
now published under the above title. In an Introduction, extending
to seventy-nine pages, the author, after explaining the general prin-
ciples of reduction employed by him, gives a list of the stars con-
tained in the Catalogue, classified into zones, as in the original
observations. The total number of zones is 52, and the total
number of stars is 4673. The Introduction is followed by the
Catalogue. The epoch as mentioned in the title is 1790·0.
There is also a supplementary catalogue of 339 stars, founded
on 13 zones, contained in the *Histoire Céleste*, but omitted in the
Catalogue of the British Association, and on two zones in the
Memoirs of the Academy of Sciences. The stars of this latter cata-
logue are all very near the pole, for in seven of the zones the mean
declinations are included between 74° and 80°, and in eight of
them between 80° and 90°. The stars are, also, in general, very
small.

The U.S. Naval-Astronomical Expedition to the Southern Hemi-
sphere during the years 1849–52, Lieut. J. M. Gilliss, Super-
intendent. Vols. I. and II. Washington, 1855.

It is generally known to astronomers that Dr. Gerling, of
Marburg, having, in 1847, suggested the expediency of making
observations of. *Venus* near her inferior conjunction at different
stations on the earth's surface, for the purpose of obtaining a new
determination of the solar parallax, Lieut. Gilliss, U.S.N., to whom

B

Dr. Gerling had communicated his views, warmly adopted the pro-
ject, and succeeded in inducing the government of his country to fit
out an expedition for the purpose of carrying it into effect.   The
Republic of Chile, in South America, was selected as likely to offer
a suitable locality for making observations of the planet, to be used
with simultaneous observations, which it was contemplated to
execute at the National Observatory, Washington.   The superin-
tendence of the operations in Chile was intrusted to Lieut. Gilliss,
whose merits as a practical astronomer had been amply established
by his labours in connexion with the United States Survey.   The
expedition sailed from the United States in the autumn of 1849.
Having erected an observatory at Santiago, upon his arrival in
Chile, Lieut. Gilliss was enabled to commence astronomical obser-
vations in the month of December of the same year.   Besides
observations of *Venus* near her inferior conjunction, the plan of
his labours included observations of *Mars* near opposition, to be
used also for obtaining a value of the solar parallax, and an exten-
sive series of observations of stars in the southern hemisphere.   It
was also contemplated to make a series of meteorological and mag-
netical observations.   The expedition returned to the United States
towards the close of 1852, after an absence of three years and
three months.

A work embodying the results of this important expedition
has been drawn up by Lieut. Gilliss, and is now being published
at the expense of the Government of the United States.   Vols. I.
and II. have already appeared.   They relate to the geography,
climate, &c., of Chile.   The remaining volumes, which will be
devoted to the main objects of the expedition, will also soon be
published.

The principal instruments used in the expedition consisted of
two refractors equatoreally mounted, a meridian circle, a clock,
and three chronometers.   The larger equatoreal, with which the
differential and micrometrical observations were executed, had a
focal length of $8\frac{1}{2}$ feet.   The object-glass by Fitz, of New York,
had a clear aperture of $6\frac{1}{2}$ inches.   The meridian circle was exe-
cuted by Pistor and Martins, of Berlin.   It had a focal length of
6 feet, and an aperture of $4\frac{1}{3}$ inches.

The weather sometimes continued extremely favourable for
observation during a long succession of nights.

"It was a great satisfaction," says the author, "to work with
an instrument like ours, but there was almost too much of it.   Out
of 132 consecutive nights after the equatoreal was mounted there
were only seven cloudy ones!"

Again, alluding to the state of the weather during the last
summer of the expedition, the author says: "Between the 15th
of December and the 15th of March, I had observations of *Mars*
on seventy-eight nights! and out of one hundred and fifty-two,
between November 10th and April 10th, there were observations
with the meridian circle also on one hundred and twenty nights!

Thus we had the satisfaction to accumulate from a previously unexplored, or almost unexplored field, an amount of astronomical data which has probably never been equalled within a similar period of time."

It ought to be stated, however, that on other occasions the weather continued cloudy for several months in succession.

At page 81 of vol. i. we find the following description of the zodiacal light:—

" These are the months* when the zodiacal light is brightest, and its perfectly formed pyramid is most distinctly traceable in the evening twilight. In no other part of the world have I ever remarked it so well. It is a pyramidal, or rather a lenticular body of light, which appears in the plane of the sun's equator, and is consequently inclined to the horizon after sunset before the vernal, and before sunrise after the autumnal equinox. The light, neither as ruddy as the glow of the sky after sunset, nor as silvery as rays heralding the moon, is usually so faint that few remark it, unless attention be directed to it; then every one wonders why it has so long escaped his attention. It is brightest about the horizon and fades gradually as it recedes ; so that it is rarely definable at a greater altitude than 40°. The base of the cone or pyramid was never more than 15°, and generally much less in diameter when its outline became discernible, its apparent breadth depending wholly on the diaphaneity of the atmosphere at the time. It was seen as early as July 6th, and is once noted in our Journal ' very bright' as late as September 6th, its place in the heavens and inclination to the horizon changing as the sun gradually advanced from his northern limit towards us. No variations in the intensity or undulatory motion of the light, such as Humboldt mentions having witnessed in the tropical regions of South America, were ever seen by me in Chili ; but only a mild radiance, whose brightness sensibly increased as the twilight faded, and more slowly disappeared an hour later ; and by eight o'clock in the early days of September, (corresponding with our March) it was no longer perceptible to eyes that had been so greatly taxed. Whether this beautiful phenomenon consists of a ring of nebulous matter, revolving freely in space between the orbits of *Mars* and *Venus*, or is the outermost stratum of the solar atmosphere, is a question yet to be decided by physicists."

The author makes the following remarks upon the transparency of the atmosphere at Santiago : " Late in the season a sort of dry fog, resembling thin smoke, deprives the atmosphere by day of something of its transparency, though the nights are all that the astronomical observer can desire. Then the Andes, whose crests are not less than eighteen miles distant in an air-line, look almost within stone-throw, and the stars rise over them with a steadiness and brilliancy known in our climate only at mid-heaven. The

* The autumnal months.

observer will appreciate me when he is told that I have made very fair micrometrical measurements of *Venus,* when the planet was not more than 3° above the eastern horizon, and its crescent was more than once seen with the naked eye."

In a separate publication entitled, *An Account of the Origin and Progress of the Expedition,* which is intended to form part of one of the succeeding volumes, we find the following notes on the variations of *η Argus,* extracted from the author's journal :—

"1850, Feb. 9. A bright, clear night, and steady atmosphere. Comparing the brightest visible stars, they rank— *Sirius, Canopus, η Argus, α Centauri.* The yellowish-red light of *η Argus* is more marked than that of *Mars.*

"Feb. 13. *η Argus* apparently less bright than *α Centauri.*

"March 31. *η Argus* is quite as bright as the two stars of *α Centauri,* and superior to all except *Sirius* and *Canopus.*

"April 15. *η Argus* approaches the brilliancy of *Canopus.*

"April 18. Night cloudless and without haze. After the observations, careful estimation placed *η Argus* quite equal with, if not superior in brightness to *α Centauri.* They were equidistant from the meridian.

"May 14. *η Argus* and *α Centauri* being at the same distance from the meridian, the former is considerably the brighter.

"May 16. *η Argus* more brilliant than the two stars of *α Centauri* combined.

"May 28. *η Argus* goes on increasing steadily. With the ruddiness of *Aldebaran,* its magnitude is only less than *Canopus.* Its change, since the close of October, has been nearly, if not full half a magnitude.

"June 3. To the unassisted eye the atmosphere is remarkably clear, the 'coal-sacks' of the *via lactea* being of startling blackness. *η Argus* and *Arcturus* have approximately the same colour; and although the latter is rather ruddier, the former is considerably the brighter. When compared, they had about the same altitude.

"July 5. *η Argus* is still as bright as the two stars of *α Centauri.*

"July 25. *η Argus* as bright or brighter than the two of *α Centauri.*

"July 28. *η Argus* is on the wane, and is now very little superior to *α Centauri.*

"1851, May 18. *η Argus* certainly not so bright as *α Centauri.*

"December 28. *η Argus* quite as bright as *α Centauri.*

"1852, January 22. *η Argus* no brighter than *α Centauri.*

"May 10. *η Argus* continued fully as bright as the double star *α Centauri* as late as the 15th of March last. Some nights I would estimate it a shade brighter ; but when the two were at equal distances from the meridian, it was often impossible to detect any difference with the eye. Since then it has sensibly diminished. It comes nearer to my recollection of *Capella,* as seen in the northern hemisphere, than any other star.'"

It may be remarked, in conclusion, that the two volumes already published contain a large mass of valuable and interesting information on the subjects to which they refer, accompanied with beautiful illustrations, and give promise that the work when completed will redound to the reputation of the author, and to the munificent liberality of the Government of the United States.

Beobachtungen des Bielaschen Cometen im Jahre 1852 *angestellt am grossen Refractor der Pulkowaer, Sternwarte, von O. Struve. Aus den Mémoires de l'Académie Impériale des Sciences de St. Petersbourg. Sixième série, Sciences Mathématiques et Physiques.* Tome vi., St. Petersbourg, 1854.

In this paper, M. Otto Struve gives an account of his observations of Biela's Comet, made with the great refractor of Pulkowa, on the occasion of the last apparition of the Comet in 1852. The first observation of the comet, that of the south following head, was obtained at Rome by Professor Secchi on the 25th of August. The comet was last seen in the Pulkowa refractor on the 28th of September. On this occasion it was the north following head which was visible. The comet was observed only at Rome, Cambridge, Berlin, and Pulkowa. The whole number of distinct observations of one or both of the heads of the comet, obtained during the period of its visibility, amounted to twenty-eight. Only five double observations were obtained, namely, three at Pulkowa and two at Rome. The following are a few extracts from the descriptive notes of M. Otto Struve, accompanying the details of the apparent positions of the comet as determined at the Pulkowa Observatory :—

" Sept. 18. Only one of the heads of the comet was recognised. This was the north preceding head, B. Its apparent diameter amounted to at least 30″. The nebulosity exhibited a considerable increase of brightness towards the centre, but there was no decided indication of a nucleus.

" Sept. 20. Both heads of the comet are distinctly seen. The head B is a little brighter than A, and has also a determinate nucleus. The nucleus of A is not so distinct as that of B. There is seen extending from it an emanation of bright nebulous matter in the direction of B.

" Sept. 23. A is to-night decidedly fainter than B, and exhibits no vestige of a nucleus. The unfavourable state of the atmosphere renders it difficult to perceive the elongated form of A. It is, however, evident that the nebulosity is not uniformly distributed about the point of maximum brightness. The nebulosity of B, on the other hand, is symmetrically situate with respect to the nucleus.

" Sept. 25. A is to-night decidedly fainter than B. A is round, B is somewhat oblong. The brightest part of A is not, situate in the centre of the nebulosity, but is turned *away* from B. The nucleus of B, on the other hand, is turned *towards* A.

" Sept. 28. There being bright moonlight and a strong twilight, B was recognisable only with great difficulty."

From the observations of September 20, 23, 25, the author deduces the following results for the apparent distance of the two heads of the comet, corresponding to the mean distance of the sun from the earth :—

Date.	Distance.
Sept. 20	43 2·5
23	42 16·2
25	41 54·2

The emanation of luminous matter from B coincided very nearly in direction with the line joining the two heads. The author is inclined to infer from this fact, that the phenomenon may be regarded as an indication of the effect produced by the action of the two heads upon each other, which was so strikingly illustrated by the interchanges of brightness exhibited by them during the period of their visibility.

Memoir of the celebrated Admiral Adam John De Krusenstern, the first Russian Circumnavigator. Translated from the German by his daughter, Madame Charlotte Bernhardi, and edited by Rear-Admiral Sir John Ross, C.B., &c., with a Portrait and Correspondence. London, 1856.

The subject of this Memoir was a native of Esthonia, one of the Baltic provinces of Russia. He was born in 1770. In early life he served several years in the British Navy. His subsequent career is one of the noblest which are recorded in the annals of any age or country. His labours in hydrography have obtained for him a place among the highest cultivators of nautical science. The editor has given a list of the works published by him at different times during his career. The most important of these is his *Atlas de l' Ocean Pacifique* (1823–6), in two vols. folio. The number of charts is thirty-four. A detailed enumeration of them is given by the editor. Krusenstern was an associate or corresponding member of various scientific societies, including the Royal Society and the Institute of France. He died in 1846, leaving behind him a reputation for exalted moral worth and professional eminence, which will always assure him a high place among the illustrious men of his country.

Professor Colla, Director of the Observatory of Parma, has requested the Astronomer Royal to transmit his thanks to the President and Council of the Society, for the Society's Catalogue of Stars, which was recently presented to him. He has also forwarded an extract from the *Gazette* of Parma, in which he makes known to the Italian public the liberality of several of our institutions to his Observatory. In this publication, he acknowledges the receipt of ten Catalogues of Stars, presented to him by different scientific bodies in this country. One of these, as has been already mentioned, was presented to him by this Society. Five were donations from the Royal Observatory, viz., Airy's Catalogues of 1439, and 2156 stars; Groombridge's Catalogue of Circumpolar Stars; Fallows' Catalogue of Stars; and Maskelyne's Ledgers of Stars. Three Catalogues were presented to him by the British Association, viz., Lalande's Catalogue of Stars, Lacaille's Catalogue of Southern Stars, and the Catalogue of 8377 Stars published by the Association. Finally, one Catalogue was presented by the Royal Society, viz., Cooper's Catalogue of Stars near the Ecliptic. Professor Colla acknowledges his obligation to the Astronomer-Royal for the exertion of his influence in procuring for him these invaluable adjuncts to his Observatory.

M. W. Struve has recently communicated to the Academy of Sciences of St. Petersburg a Memoir on the great Nebula of *Orion*, by M. Liapounov, Director of the Observatory of Kazan. The author's labours extended over a period of four years. His observations were made with an equatorially mounted refractor, equal in optical power to the Dorpat Instrument, and a meridian circle by Repsold. The places of all the stars in the nebula were rigorously determined by a process of triangulation, and its physical features delineated with the most scrupulous care. The author then institutes a comparison between his results and the anterior representations of the nebula by Lamont, Sir John Herschel, and Bond. According to M. Struve the observations of M. Liapounov would seem to indicate that the nebula *is subject to changes of form and relative brightness in its different parts.*

There is at present for disposal a first-class object-glass, by Merz, of Munich, 6¼ in. clear aperture, 8½ feet focal length, *with* or *without* tube and powers. Apply at the Apartments of the Society.

CONTENTS.

ROYAL ASTRONOMICAL SOCIETY.

MANUEL J. JOHNSON, Esq., President, in the Chair.

Rev. W. Jackson, M.A., St. John's, Workington; and
Geo. Roberts Smalley, Esq., Head Mathematical Master in
King's College School,
were balloted for and duly elected Fellows of the Society.

*On the Values of the Diameters of the Planets having measurable
Disks, as determined with a Double-Image Micrometer attached
to the East Equatoreal of the Royal Observatory, Greenwich.
By the Rev. R. Main, M.A.*

"The observations, of which the results are here discussed,
extend over a long space of time,—namely, from the time of the
invention of the double-image micrometer by Mr. Airy in 1840
till the year 1851. At the commencement of the use of this
instrument, it was considered that one of the most important appli-
cations of it would be for the determination of the diameters of
planets, its range extending considerably beyond that required for
the measurement of the system of *Saturn*, and its accuracy within
that range being far superior to that attainable with a wire-
micrometer. In former papers I have discussed those observa-
tions made by myself for determining the ellipticity and the form
of *Saturn*, as well as those for determining the magnitude and the
breadth of his bright rings; and I have thought it of importance
at the present time to collect all the measures of the other planets
made by myself, and to give definitively the results for all, as ob-
tained from a series of observations made by the same person
with the same instrument and in a uniform way.

"The instrument with which the observations were made was
not identically the same during the whole period of time. The
chief changes which occurred were in the years 1849 and 1851,
when advantage was taken of some arbitrary conditions in the ratios
of the focal lengths and distances of the lenses, to ensure greater
flatness of the field and greater distinctness of the images near the
borders. In this latter construction the power is altered by the
change of the eye-glass nearest the object-glass of the telescope.
"Previously to the year 1849, the value of a revolution of the

micrometer-screw, as determined by observations admitting of no doubt, was 17″·00. For the power used with the new eye-piece in 1849, commencing with February, the value of the revolution was assumed to be 14″·59, and, finally, for that constructed according to M. Valz's arrangement of the lenses in 1851, the value of a revolution for the power actually employed was 7″·23."

After a few additional remarks, the author proceeds to give the details of the measures which formed the groundwork of his researches. The remaining part of his paper is devoted to a discussion of those measures and the deducing of the definitive results.

Mercury.—This planet is so rarely visible in the evening, and so difficult to be measured in full daylight, that the author's materials for determining its diameter were not very abundant, although they are probably sufficient for obtaining a moderately accurate result. The definitive value of the diameter corresponding to the unit of distance is found to be 6″·89.

Venus.—In the discussion of the measures of this planet, the author takes into account the probable effects of irradiation. Considering, first, the daylight observations, he separates the measures into two classes—those made near inferior conjunction, and those near superior conjunction ; and then forms two systems of equations of condition involving the constant of irradiation and the diameter of the planet as the unknown quantities. By a simple combination of the equations of condition in each set, he obtains two final equations, by the solution of which he finds the apparent true diameter of the planet corresponding to the unit of distance to be 17″·61, and the constant of irradiation to be −0″·61. A rigorous treatment of the equations by the method of least squares gives him 17″·55, and −0″·50 for the values of the same quantities. It would appear from the resulting value of the constant of irradiation, that the measured diameters are in reality too small by 0″·5, and that the contacts have been made too closely by the quantity 0″·25. This amounts, in fact, to saying, that on account of the feebleness of the light at the cusps and borders of the planet, the true cusp or border is not observed, but a point lying within it by 0″·25, a result which might have been in some degree expected. A discussion of the evening measures of the planet shows them to be totally free from the effects of irradiation,—a startling and unexpected result for a planet apparently surrounded with so large a mass of coloured diffused light.

The author next proceeds to discuss the measures for the breadth of the illuminated disk. The results for the daylight observations exhibit a most satisfactory agreement with that deduced for the measured diameters, while the evening observations show that the portion of the disk measured is also less than the true illuminated breadth, though by a smaller quantity. " Hence," says the author, " on the whole we may affirm that for evening measures of *Venus*, made with the double-image micrometer and telescope here employed, no correction is needed for the

effects of irradiation at the limb, and that at the boundary of light and darkness the faintness of the light causes the contacts to be made at a distance $0''\cdot36$ from the point where the sun's light really touches the planet."

Mars.—The author finds the diameter of this planet at the mean distance to be $9''\cdot84$. He has also obtained $\frac{1}{62}$ for the ellipticity of the planet—a result which he considers to be a tolerable approximate to the true value.

Jupiter.—The series of observations made for the determination of the size and form of this planet is very extensive, and a synopsis of the results is given in a table, which shows the degree of agreement of the separate determinations. In this table the corrections due to the want of illumination of one or the other equatoreal portion of the disk are given, but not applied to the equatoreal diameters; but they are taken into account in the calculation of the ellipticity and of the equatoreal diameters at the standard distance. This distance has been assumed so as to be identical with M. Struve's value, in his paper on the measures of *Jupiter* and *Saturn* (*Mem. R. A. S.* vol. iii.) namely, $5\cdot20279$. The ellipticities given in the table express, as usual, the values of —

$$\frac{\text{Equatoreal diameter} - \text{Polar diameter}}{\text{Equatoreal diameter}}$$

and their general agreement is as satisfactory as can be desired, and prove abundantly that the ellipticity $\frac{1}{13\cdot8}$ assigned by M. Struve is too large. As an additional proof of the accuracy of his definitive value of the ellipticity, namely, $0\cdot05934$, or $\frac{1}{16\cdot84}$, the author has calculated by means of it and the observed value of the equatoreal diameter, on each day when oblique diameters were also observed, the values of the oblique diameters, and has compared them with the observed values. The calculation is made in the following way.

Let a and b be the equatoreal and polar semi-axes of any ellipse, and r the radius-vector (referred to the centre) inclined at an angle θ to the major axis : —

Then

$$r^2 = \frac{a^2 b^2}{a^2 \sin^2 \theta + b^2 \cos^2 \theta}$$

Let ϵ be the ellipticity

$$= \frac{a-b}{a} = 1 - \frac{b}{a}$$

therefore $b = (1 - \epsilon) a$
and r^2 (after reduction)

$$= \frac{a^2 (1 - \epsilon)^2}{1 - (2\epsilon - \epsilon^2) \cos^2 \theta}$$

whence

$$r = a(1 - \epsilon) \left\{ 1 + \left(\epsilon - \frac{\epsilon^2}{2}\right) \cos^2 \theta + \frac{3}{2} \epsilon^2 \cos^4 \theta \right\}$$

Now if, as is the case for very nearly all the observations of oblique diameters, $\theta = 45°$, then,

$$r = a(1 - \iota)\left(1 + \frac{1}{2}\iota + \frac{1}{8}\iota^2\right)$$

or,

$$2r = 2a\left(1 - \frac{\iota}{2} - \frac{3}{8}\iota^2\right)$$

or oblique diameters at angle $45°$

$$= \text{equatoreal diameter} \times \left(1 - \frac{\iota}{2} - \frac{3}{8}\iota^2\right).$$

If, then, we assume $\iota = \frac{1}{16.84}$, we have,—

Oblique Diameter $=$ Equatoreal Diameter $\times (1 - 0.03102)$

and by this formula the computed oblique diameters have been derived from the observed equatoreal diameters. These have been compared immediately with the means of the two transverse diameters corrected for phase, and the results are contained in the following table : —

Date of Observation.	Mean of Observed Transverse Diameters.	Computed Transverse Diameter.	Excess of Computed Diameter.
1840, May 16	42·65	43·25	+0·60
June 10	41·70	41·30	—0·40
1841, Feb. 19	34·88	34·51	—0·37
1842, July 15	46·57	46·86	+0·29
1849, Feb. 17	42·80	43·14	+0·34
Feb. 26	42·33	42·37	+0·04
Feb. 27	42·25	42·26	+0·01
Mar. 20	40·85	40·24	—0·61
Mar. 21	40·41	40·27	—0·14
Apr. 7	39·00	38·95	—0·05

The mean of the excesses of the computed above the observed diameters is, therefore, $-0''.03$, which shows, in the first place, that the ellipticity has been rightly assumed, and, in the second, that the planet is strictly of a spheroidal form. Hence the author assumes definitively,—

Equatoreal diameter at distance $5.20279 = 37''.91$

— — at distance unity $= 197.24$

Ellipticity $= \dfrac{1}{16.84}$

After a few remarks on the general smallness of the measures of 1849, the author concludes with a synopsis of the several definitive results contained in his paper.

*Extract of a Letter from the Rev. W. R. Dawes to the
Astronomer Royal.*

" I have the pleasure of inclosing an account of my observation
of the lunar occultation of *Antares* on the 26th, which was as
satisfactory as the wretched condition of the atmosphere would
permit. From many other observers, as well as from myself, thanks
are due to you for so pointedly calling attention to this pheno-
menon. I confess I was not previously aware of the peculiarities
you mention as having been formerly noticed in the occultations
of this star.

" *Wateringbury*, 1856, *March* 31."

On an Occultation of Antares by the Moon, 1856, *March* 26.
By the Rev. W. R. Dawes.

" At 15^h 20^m G.M.T. I directed my equatoreal refractor of 8 in.
aperture to *Antares*, which the bright edge of the moon was
approaching. The state of the air was very bad, the star being
often puffed out to an enormous size, and exhibiting various bril-
liant colours. The moon's edge was boiling. After trying several
different magnifiers, an equiconvex lens producing a power of 254
was preferred, because with it the small companion was occasionally
seen separated from the large star, when, for a few moments, the
latter drew itself up into smaller compass. The air would not bear
a higher power; and with a lower the small star was constantly
enveloped in the blaze of the large one.

" The *time* of the disappearance was not very accurately noted,
my attention being directed almost entirely to the appearance of
the star. Nothing decidedly remarkable was noticed previous to
the disappearance of *Antares*, which took place instantaneously,
and precisely at the moon's edge. There was a small depression
in the edge at the point of disappearance. The small companion
was not discerned at all for about one minute previously. The
moon was quite free from cloud.

" The reappearance at the dark edge of the moon was well
observed. The equatoreal was not used between the disappearance
and reappearance; and being well carried by its driving clock, the
phenomenon occurred in the centre of the field, and was seen at
the first instant. The small companion appeared instantaneously
at its full brightness; but from its diffused condition, it was not
possible to form any correct judgment of its magnitude, which,
however, could scarcely exceed the 7th. *Its bluish green colour
was very conspicuous.* When I had counted about 18 beats of my
pocket-chronometer, equal to 7^s·2, *Antares* itself suddenly blazed
forth, and completely enveloped its companion in its enormously
diffused image. Vision was even worse than at the disappearance;
and I could not afterwards obtain a glimpse of the companion,
though the strong twilight favoured its visibility.

" I fear there is some uncertainty in the observed interval: for
my attention was so absorbed with examining the colour of the

small star, that the counting was carried on mechanically; and the startling effect of the burst of light at the close increased the tendency to error. But I hope the uncertainty does not exceed one or two beats of the chronometer.

"From the decidedly blue-green tint of the small companion, while *Antares* was still occulted, it seems evident that the colour really belongs to the star, and is not merely the effect of contrast with the deep red of the large star. As far as *artificial* occultation can decide the point, this seems to be the case with many other double stars whose components show a somewhat similar contrast of colours. In the present instance it was a peculiarly favourable circumstance that the *re*appearance happened at the moon's *dark* limb, as the possibility of the judgment of the eye being influenced by the light and colour of the moon itself is thereby precluded. To secure this, I employed a small field of view in the sliding diaphragm of my solar eye-piece.

"From the interest attaching to this object, and the extreme difficulty of procuring reliable measures of it as a double star in these latitudes, I am induced to anticipate the publication of the results I have been able to obtain of some observations of its position and distance. Having accidentally heard, in the spring of 1847, that *Antares* had been discovered to be close double by Professor Mitchell of Cincinnati, but being unacquainted with any particulars respecting the relative proportion of the components or their angle of position, I determined to try, on the first favourable night, what could be made of it with my 6½-in. Munich refractor. The following are the results which I obtained in that and the two subsequent years :—

	Pos.°	Obs.	Wt.	Dist.″	Obs.	Wt.	Power	Mags.	Epoch
a)	273·98	2	4	...	...	...	435	1½.8	1847·297
b)	273·63	9	67	3·471	10	42	435	1½.7½	,, ·299
c)	270·08	5	19	3·642	4	14	252	1½.8	1848·557
d)	271·62	5	20	3·418	4	16	252	1½.7	,, ·591
e)	275·97	5	20	3·247	4	12	375	1½.8	1849·406

Notes.

a) A, *red*; B, *blue.* No doubt about the companion. Became foggy. Observation worth little.

b) A, *red*; B, *purple?* Surprisingly well seen: a most beautiful and delicate object.

c) A, *red*; B, *purplish.* Daylight.

d) A, *red*; B, *very blue.* Daylight. Beautiful object.

e) A, *red*; B, *green.* Occasionally very well seen, but very unsteady.

Mean Result.

P=273·17; obs. 26; wt. 130; on 5 nights ⎱ Mean Epoch, 1848·02
D=3·457; ,, 22; ,, 84; ,, 4 ,, ⎰

"If the angle of position were nearly coincident with the meridian, it would be almost impossible to observe the small star

in these latitudes, as the bright star forms a strong prismatic spectrum in that direction. This atmospherical effect may, however, be in a great measure counteracted by using a single lens as an eye-glass, or by the ordinary double micrometer eye-piece. The star being placed towards the southern (upper) side of the field of view, the eye-piece spectrum may be made very nearly to neutralise the atmospheric spectrum, and a very tolerable image obtained under favourable circumstances. The star was always thus treated while under measurement; otherwise no reliable measures of position could have been procured.

" *Wateringbury, Maidstone.*"

Note on the Occultation of Antares, observed 1856, *March* 26,
at the Royal Observatory, Greenwich, by Mr. H. Breen.

The instrument used was the telescope of the altazimuth, a 5-feet telescope of 4 inches aperture. Nothing remarkable was noticed at the disappearance ; the star disappeared instantaneously, although it was connected with the bright limb for several seconds. At the reappearance, the star was at first seen as a small point; and several seconds (probably 10 seconds) elapsed before it showed itself in its full lustre.

Note on the Occultation of Antares, March 26th, 1856.
By S. C. Whitbread, Esq. F.R.S.

" Soon after three o'clock, mean time, on this morning, I had a very good view of the occultation of *Antares* by the moon. The disappearance behind the bright limb of the moon was instantaneous, as if the star had been swallowed up. On the reappearance taking place, I saw a small speck emerge from behind the dark limb, which I should describe like a star of the twelfth magnitude, and in seven seconds *Antares* burst forth in full splendour.

	h m s
The disappearance took place at ...	15 43 33
Reappearance — —	17 2 16
The latitude of my observatory is ...	52° 6′ 0″ N.
Longitude — —	0ʰ 1ᵐ 39ˢ W.

" *Observatory, Cardington, near Bedford;*
" *March* 27, 1856."

Measures of the Exterior Diameter of Saturn's Ring.
By Professor Kaiser.

(*Extract of a Letter to the Editor.*)

" At the end of last year I received the double-image micro-
meter which I had ordered of Mr. Simms, but different circum-
stances prevented me for a few months from making use of it.
A favourable opportunity for employing it first offered itself in
the month of March, when I resolved to measure the planet
Saturn with it.   On the 10th of March the sky was tolerably
transparent, but cloudy weather then ensued, and it was not
until the 25th of the same month that at length there occurred
a succession of several clear days.   During this time, however,
there blew incessantly a strong wind from the north or north-
east, accompanied with intense cold; and the air, which was
very transparent, was affected with such violent undulations,
that *Saturn* appeared like a confused mass of light, so that to
execute any exact measures was altogether impossible.   Under
such circumstances, the complete measurement of *Saturn* with
the double-image micrometer being impracticable, I was obliged
to wait for more favourable weather, in order to examine M.
Otto Struve's theory by my own measures.   I thought, however,
that in the meantime I might be enabled to contribute something
to the examination of the hypothesis communicated by Professor
Secchi in the *Monthly Notices*, vol. xvi. No. 3.   For this I only
wanted to measure the exterior major axis of the outer ring,
which is by far the easiest to measure; and I thought that a
change in a few hours of nearly a whole second, if it existed, could
not fail to reveal itself also to me, notwithstanding the unfavourable
condition of the atmosphere.*
    " If the air was tranquil for a moment, the micrometer exhibited
pretty distinct images ; the instrument, however, cannot be used
for minute measurements without great circumspection.   A prin-
cipal difficulty appears to me to consist in the circumstance that
the result of the measurement depends on the point of the field of
view in which the images coincide.   The variability hence arising
increases with the distance to be measured, and may for a distance
not exceeding two minutes produce an error of a considerable
part of a second.   With the highest magnifying powers, the mea-
surement can therefore hardly be extended to two minutes, but
the influence of the variability upon the following measures of
Saturn is certainly very small.
    " Supposing, that the value of a revolution of the screw amounts

* The author here alludes to an inconvenience which he has experienced in
endeavouring to determine by the aid of a wire-micrometer the value of a revo-
lution of the screw for a high magnifying power.   Mr. Airy in the description of
his double-image micrometer (Introduction to *Greenwich Observations*, 1840),
has shown how the value of a revolution of the screw may be obtained inde-
pendently of the use of a wire-micrometer.—EDITOR.

to 6″·430, I obtained for the major axis of the ellipse formed by the
outer edge of the exterior ring of *Saturn* the following results :—

1856.	M. T. Leyden.	Major Axis Observed.	Major Axis Reduced to Mean Dist.	Deviation from the Mean.
	h m	′	′	″
March 10	9 23	42·26	39·46	+0·04
	9 48	42·12	39·33	+0·17
25	8 50	40·87	39·23	+0·27
	9 35	41·05	39·40	+0·10
	10 12	41·06	39·41	+0·09
26	7 15	41·21	39·62	−0·12
	9 17	41·30	39·71	−0·21
	10 6	41·20	39·61	−0·11
27	8 32	41·02	39·51	−0·01
	9 41	41·14	39·63	−0·13
	10 12	41·21	39·69	−0·19
28	7 40	40·96	39·52	−0·02
	8 57	40·65	39·22	+0·28
	10 0	40·93	39·49	+0·01
29	7 20	40·80	39·44	+0·06 .
	8 40	41·07	39·70	−0·20
30	7 20	40·99	39·69	−0·19
	8 40	40·86	39·56	−0·06
31	7 20	40·73	39·50	0·00
	8 35	40·77	39·54	−0·04

Mean, 39·501

" The second measure of the 10th of March was executed by
Dr. Oudemans; all the others are by myself.

" All the above measures have been executed under the most
unfavourable atmospheric conditions, and it is not to be doubted
that the double-image micrometer admits and will furnish in
future much more exact measures. It is remarkable that all the
above measures accord incomparably much better with each other
than those which Professor Secchi has communicated in the
Monthly Notices, vol. xvi. p. 52. After the application of a cor-
rection, that diminishes the differences between his results not less
than 0″·732, the latter continue to exhibit deviations from the
mean, which are not less than those of my measures without the
application of any corrections. My measures, therefore, do not
seem to indicate any trace of a fluctuation by which the apparent
major axis of the ring increases or diminishes to the extent of
0″·732 in an interval of 7ʰ·214. A fluctuation in a period of
7ʰ·214 may remain concealed for weeks if the ring is measured
daily at the same hour ; for if the ring is measured the first day
just in its mean value, this will be nearly the case for many days
together at the same hour, and the fluctuation may exist without

disclosing itself even to accurate measures. My measures, how-
ever, have been executed at sufficiently different hours to make
such a concealment of the fluctuations almost impossible, and it is
not probable that the errors of my measures have followed with
precisely the same magnitude the same period in an opposite direc-
tion as the change which Secchi thought he had discovered in
the major axis of the ring of *Saturn*. I hope to execute under a
better condition of the atmosphere more complete and more ac-
curate measures of *Saturn*, but the result of this preliminary
inquiry appeared to me sufficiently interesting to communicate it
briefly to you.
        "*Leyden, April* 3, 1856."

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*Occultations Observed at the R.N. College, Portsmouth,
by Capt. Shadwell, R.N.*

M.T. Portsmouth.

1856				h m s
Feb. 16	45 Geminorum	Mag. 6	Immersion. Dark Limb	11 52 36
Mar. 11	33 Tauri	Mag. 6	Immersion. Dark Limb	9 27 54
„ 13	136 „	Mag. 4½	Immersion. Dark Limb	8 48 31
„ 26	α Scorpii	Mag. 1½	Immersion. Bright Limb	15 20 50·5
			Emersion. Dark Limb	16 39 50·5

All good observations. In the case of α *Scorpii*, the star at
the immersion appeared to hang on the moon's limb for about two
seconds; the time recorded is that of final disappearance. The
reappearance was instantaneous.

Note on a presumed Occultation of a Star by Saturn.
By Frederick Brodie, Esq.

   " On 30th January observed *Saturn*, and was surprised to
find that there were apparently four satellites plainly visible
interior to *Titan*. I could glimpse a fifth occasionally. After
a period of 2¼ hours, I perceived that the one nearest the planet,
then about 30″ distant from the edge of the ball, had not moved,
but rather approached the planet; while the satellite next to it,
Tethys, had altered its position with respect to it nearly 90°. I
suspected it therefore to be a star, its brightness and magnitude
coinciding exactly with one of the interior satellites.
   " On 31st January I observed this supposed star about 100″
distant from the edge of the ball of *Saturn* but on the opposite
side; the satellites, *Enceladus, Tethys, Dione, Rhea*, were visible,
and in the respective proper positions due to their daily revolu-
tions. This star had been occulted by the planet during the inter-

val between the observations; but such occultation would not have been visible in England, as the planet would have set, before the immersion took place.

" The apparent magnitude of the star was 9 to 10.

" *Eastbourne, Feb.* 7, 1856."

Notes on an Occultation of a Star by Saturn.
By the Rev. W. R. Dawes.
(*Extracts of Letters to the Astronomer Royal.*)

" Last evening, on turning my equatoreally-mounted telescope upon *Saturn*, I perceived a small fixed star on the preceding side of the planet, and almost precisely in the line of the major axis of the ring produced. Allowing for the brilliancy of *Saturn*, I estimated it be of the 9th magnitude. It was distant from the western extremity of the ring about 40″. The third satellite, *Tethys*, was very near its greatest elongation on the same side.

" As it appeared evident that an occultation must happen, I measured with the wire micrometer the difference of R.A. of the star and the western extremity of the ring; and from this, and *Saturn's* daily motion in R.A., I found that the G.M. time of occultation by the ring would be about 15ʰ 6ᵐ. Having also observed the difference of N.P.D. of the star and the most southerly point of the edge of the ring (considerably to the west of the southern extremity of its minor axis), I found from this and the daily motion in N.P.D., the difference of N.P.D. at the expected time of occultation. The threads of the micrometer having been set to include this difference of N.P.D., and placed equatoreally, the southern thread was made to touch the southern point of the ring; and the other consequently showed, with sufficient exactness, the point on the edge of the ring where the occultation would take place, and also indicated the path of the star behind the planet. It thus became evident that the star would disappear a little to the south of the western extremity of the major axis; and that it would not make its appearance at all in the interval between the ring and the planet, but would just graze the southern edge of it; allowance being made for the northward motion of *Saturn*, amounting to about 0″·18 in an hour. Though the interest of the phenomenon was thus greatly diminished, I thought it worth while to observe the occultation with the view of ascertaining whether so small a star would continue to be distinctly visible up to the very edge of the ring; and also, with some hope, that if the planet should be sufficiently well seen when about 7ʰ west of the meridian, the star might appear between the bright rings.

" At 11ʰ 25ᵐ, G.M.T., *Tethys* was very nearly south of the star, and at a distance of about 5″. The star was estimated to be rather brighter than the fifth satellite, *Rhea*.

" At 14ʰ 50ᵐ, G.M.T., *Saturn* had approached very close to

the star; which, however, was still at times distinctly separated from the edge of the ring: but usually the edge was sadly tremulous. Power 162.

" At $15^h 1^m \pm$ I had the last distinct and certain view of the star during a few moments of steady vision. It was just in contact with the edge, and looked like a huge mountain projecting from it. Great tremulousness of the image precluded another view of the star. The last-mentioned time is a little uncertain; for I kept my eye for about two minutes at the telescope, in hopes of one more firm glimpse; which, however, did not occur.

" The state of the air soon afterwards became so bad, that the division between the rings was only occasionally visible; and it therefore seemed useless to await the passing of that division over the star. Had the planet been near the meridian, and as steady and well defined as it had been when in that position, I have little doubt that the star, though of so small a magnitude, might have been seen between the rings. It is to be hoped that this interesting phenomenon has been observed in America.

" When *Saturn* has removed to a sufficient distance from the star, I shall endeavour accurately to gauge its magnitude.

" The telescope employed in these observations has a focal length of nearly 10 feet, with a clear aperture of 8 inches, and is the work of Mr. Alvan Clark of Boston, U.S. Its defining power is very fine.

" Hoping that this observation, though far less complete than I could wish, may yet possess sufficient interest to excuse my troubling you with the present communication.

" I remain, yours faithfully,   W. R. DAWES.

" *Wateringbury, Maidstone,* 31 *January,* 1856."

"The early part of last night was very clear here, and I took some pains to obtain an accurate gauge of the magnitude of the fixed star, which was occulted on the 30th ult. To relieve the eye from the light of *Saturn*, a very small field of view was employed: but when the star was placed in its centre, the sky around it was evidently somewhat illuminated by the planetary twilight. The aperture on the telescope (the finder of my equatoreal), which rendered it just steadily visible, gave its magnitude $9\frac{1}{4}$ of my scale. The planetary illumination of the field, though at a distance of above 9' from *Saturn's* centre, might make it appear a quarter of a magnitude smaller than the truth,—but scarcely, I think, half a magnitude. I intend, however, to resume this examination in a week or so, if opportunity should be afforded, when *Saturn* will have removed to more than twice his present distance from the star, and the moon will scarcely have become troublesome.

" I confess I was not prepared for the distinct visibility of so small a star when close to *Saturn's* ring; especially after Professor Secchi's remarks on the extreme faintness of the seventh-magnitude star, when it was very close to the planet on the 13th

Nov. 1854. (See *Astron. Nachr.* No. 982, p. 346, where it is not stated whether that star was actually *occulted* by *Saturn*.) When the star I observed was close to *Tethys*, I estimated it to be fully one magnitude, or perhaps one and a half larger than that satellite."

" *February* 4."

" I have carefully, and under favourable circumstances, gauged the magnitude of the small star, which was occulted by *Saturn* on January 30. I find it is rather brighter than the 8¼ mag. and decidedly below the 8½. *Saturn* was excluded from the field during the observations; and the sky round the star was not perceptibly illuminated by the light from the planet: yet, to make sure of not underrating the magnitude of the star, it may, I think, be safely stated decimally as 8·6.

" At the same time, and by the same method of apertures, I gauged the magnitude of the companion of *Polaris;* which resulted, as nearly as possible, one magnitude lower, or about 9¾. According to the ratio I employ, the star occulted by *Saturn* is therefore fully four times as bright as the companion of *Polaris.* Its approximate place is

" R.A. 5ʰ 32ᵐ 18ˢ; N.P.D. 67° 48′ 24″.

" *March* 31."

Mr. De La Rue laid before the Meeting a coloured drawing of *Saturn*, as seen with his 13-inch Newtonian equatoreal, on March 28 and 29, 1856. Adopting the outer diameter of the outer ring as the unit for comparison, Mr. De La Rue finds that the relative dimensions which best agree with his observations are as follows : —

Outer diameter of outer ring	1·00000
Diameter of fine division	0·95417
Inner diameter of outer ring	0·88702
Outer — — middle —	0·87099
Inner — — — —	0·66106
— — — faint —	0·55586
Equatoreal diameter of the planet	0·44338

There was also exhibited at the Meeting a drawing of *Saturn* as seen at the Observatory of Mr. Barclay, Knotts Green, Leyton. The telescope employed was a refractor of 7½ inches aperture, by Cooke of York, mounted equatoreally and driven by clockwork. The general resemblance which the drawing bore to other authenticated delineations of the planet was very satisfactory, and gave promise of future usefulness.

On the Outer Ring of Saturn. By Mr. John Watson, Washington Chemical Works, Durham.

(*Communicated by the Astronomer Royal.*)

"As the question of the division of the outer ring of *Saturn* cannot be looked upon as settled yet, it will not be necessary, I hope, to offer any apology for adding my mite of evidence to the mass already before the astronomical world. Some observers believe that the outer ring is divided in a manner similar to the well-known division between the outer and inner bright rings, but divided only by a comparatively narrow space; while others, possessed of powerful instruments, are of opinion that the outer ring, if marked at all, is only marked by a narrow *streak*, similar in appearance to the *streaks* or *belts* which so conspicuously cover the balls of *Jupiter* and *Saturn*. Some have even ventured so far as to divide the respective observers into the achromatic telescope and the reflecting telescope parties; the latter class not being able to see the division or mark, from some slight imperfection in the instrument. We ought not, however, to overlook the objections of Capt. Kater in 1825-6, with Newtonian telescopes of 6¼ and 6¾-in. aperture (*Astronomical Memoirs*, vol. iv.); nor the observation of Messrs. Lassell and Dawes, in Sept. 1843, with Mr. Lassell's 9-in. telescope. Both of these sets of observations, it will be seen, were made when the rings were pretty well opened as at present.

"The planet being in a good position during the last few months, I have paid close attention to the outer ring, with the view of judging, for myself at least, which of the opinions is most probably correct. My telescope is a 12-feet Newtonian reflector of 12-in. clear aperture, and possesses very excellent defining power. I believe very few large telescopes are able to show a star with a smaller spurious disc, as may be judged from the fact that η^2 *Andromedæ* has been neatly *split* with the full aperture, and a power of only 420, and well divided with powers from 500 to 900.

"I have obtained glimpses of the marks on the outer ring of *Saturn* on several occasions during the past winter, and my first impression was, that it was as really a division as that which separates the principal bright rings, but that it was closely bordering on the *minimum visible*, even with our best telescopes. I was led to suppose that the principal division would present much the same appearance with a telescope of barely sufficient magnifying and penetrating power to render it visible. To settle that point, I have carefully examined its appearance with a very fine 2¾-in. achromatic telescope by Cooke, using powers barely sufficient to give a steady view of the rings. For the same purpose, I have also contracted the aperture of my 12-in. telescope to only about 3 in. clear aperture (making allowance for the size of the small plane speculum) and reduced the magnifying power in the same proportion, until it was just possible to see the division by glimpses. When viewed

under these circumstances, I observed that the principal division presented the appearance of an exceedingly fine *black* line or mark, *without sensible breadth.*

"Under the most favourable circumstances, with the full aperture of the telescope, and powers varying from 300 to 900, I have never been able to detect on the outer ring that black appearance which I am led to suppose, even a very narrow division would present; and, on more than one occasion, I have obtained the impression *that the mark on the outer ring possessed sensible breadth.* I have also reason to suppose that *its appearance is variable.*

"At the conclusion of a very favourable season, my impression is, *that the outer ring of Saturn is not divided, but merely marked by a dark belt or streak of variable intensity.*

"*20th March,* 1856."

Observations of recently discovered small Planets, made at the Royal Observatory during the Month of March, 1856.

The North Polar Distances are corrected for Refraction and Parallax.

Urania.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s 1856, Mar. 7 9 5 19·0	h m s 8 8 21·52	o ′ ″ 70 20 3·08

Pomona.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s 1856, Mar. 14 9 56 28·4	h m s 9 27 15·15	o ′ ″ 83 22 18·01
31 8 45 42·2	9 23 18·82	82 4 11·01

Victoria.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s 1856, Mar. 14 10 7 49·8	h m s 9 38 38·50	o ′ ″ 89 2 40·80
26 9 14 4·4	9 32 2·88	87 45 25·24
27 9 9 44·	9 31 39·16	87 39 49·18
29 9 1 11·4	9 30 57·44	87 28 11·33
31 8 52 44·5	9 30 22·20	87 16 51·48

Leda.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1856, Mar. 27 7 44 20·9	8 6 1·22	73 49 51·95

Egeria.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1856, Mar. 14 10 57 29·3	10 28 26·14	54 12 49·57
26 10 1 10·9	10 19 17·10	55 26 37·43
27 9 56 41·1	10 18 43·16	55 34 29·33
29 9 47 47·1	10 17 40·74	55 51 9·85
31 9 39 1·2	10 16 46·57	56 8 14·78
Apr. 1 9 34 38·9	10 16 20·10	56 18 26·38

Euphrosyne.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1856, Mar. 7 11 27 1·0	10 30 26·77	42 45 33·48
14 10 51 53·8	10 22 49·68	43 38 11·52
26 9 55 5·5	10 13 10·72	45 40 58·52
31 9 32 50·1	10 10 34·38	46 41 25·77
Apr. 1 9 28 27·9	10 10 8·07	46 53 59·58

Polyhymnia.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1856, Mar. 7 9 18 56·0	8 22 0·78	68 21 49·08
29 7 48 40·2	8 18 14·26	68 24 44·65

Phocea.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1856, Mar. 26 8 36 35·8	8 54 28·09	99 7 51·81
29 8 24 23·3	8 54 3·27	98 35 49·38

Thetis.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, Mar. 7 14 39 37·4	13 43 34·80	92 6 21·68
11 14 22 35·4	13 42 16·29	91 43 39·69
14 14 9 (40)	……….	91 25 22·00
27 13 11 9·3	13 33 43·27	89 57 36·66
31 12 52 24·8	13 30 41·89	89 29 25·42
Apr. 1 12 47 41·3	13 29 54·15	89 22 25·94

Lætitia.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, Mar. 26 10 31 2·6	10 49 13·77	79 27 27·17
31 10 8 23·6	10 46 13·80	79 1 53·24

March 26. There is a little doubt whether this object is the planet.

G. B. AIRY.

1856, April 14.

Measurements of Saturn with the Astronomer Royal's Double-image Micrometer, employed with a 13-in. Newtonian Equatoreal, March 28, 1856. By Warren De La Rue, Esq.

Outer Diameter of Outer Ring.		Mean of 10 Measures Reduced to Mean Dist. Log = 0·979,6849	
Major axis	... 41·06	...	39·51
Minor axis	... 18·71	...	18·00 Mean of 6 Measures

Distance of the boundary of the equatoreal bright zone from the northern edge of the outer ring :

Mean of 6 Measures   ...   6″·94

corresponding to the 11° of south latitude.

*Ueber Dr. Wichmann's Bestimmung der Parallaxe des Argelan-
der'schens Sterns, von W. Döllen.* (Bulletin Physico-Mathé-
tique, tome xiii.)   St. Pétersbourg, 1854.

This paper is devoted to a critical examination of Dr. Wich-
mann's researches on the parallax of Argelander's star (1830 *Groom-
bridge*).   The author states that it was placed in the hands of M. W.
Struve as early as the month of June 1853, although, for a reason
explained by him, its publication was delayed for some time.

After a few preliminary remarks, the author commences by
giving a concise statement of the nature of the inquiry under-
taken by Dr. Wichmann, and of the conclusions at which he
arrived.   The observer measures the distance between two stars,
p and s, separate from each other by an interval of about 3200″.
The results obtained by measures of this kind exhibit variations
of a periodic character, and these being discussed upon the suppo-
sition of their being due to parallax, it has been found that the
difference of parallax of the two stars amounts to 1″·17 with a
probable error of 0″·08.   Again, the observer measures the distance
of the two stars p and s from a third star A (Argelander's star),
situate nearly midway between them; and the periodic variations
in the *differences* of these distances, which are very nearly equal
in quantity and direction, lead to the conclusion that the excess of
the parallax of A over half the sum of the parallaxes of p and s
amounts to 0″·135, with a probable error of 0″·013.

The author, in his examination of the reasoning by which these
results were deduced, has recourse, first, to considerations of a ge-
neral nature, and then endeavours to substantiate his views by a
process of calculation.   He commences with an allusion to the many
fruitless attempts which have been made to ascertain the parallax
of the fixed stars.   The memoir of Dr. Peters on this subject affords
a highly instructive picture of the persevering efforts of the human
mind in search of truth.   These efforts, although unattended with
success, in so far as the object of pursuit was concerned, have,
notwithstanding, proved very advantageous to Astronomy, from the
valuable improvements of the methods of investigation which they
have suggested, and the astonishing perfection which they have oc-
casioned in the fabrication of instruments of observation.

In Stellar Astronomy the progress of improvement has been
such, that the objects of investigation may now be considered to
be of the same order of magnitude as the disturbances by which
the condition of the instrument in each individual instance of obser-
vation is inevitably affected.   The peculiar feature of modern
observation consists in the circumstance that the principal difficul-
ties which offer themselves to the observer, reside not in the
heavens, but in the instrument with which his observations are
made.   These must be overcome before any ulterior success in his
inquiries can be expected.

To the astronomer the most perfect instrument is not that which is affected by the smallest errors, but that whose errors he is best acquainted with. It is not sufficient, however, that the observer has been enabled, *in any given instance*, to determine the errors of his instrument. In order to render his observations available for theoretical purposes it is desirable that he should know how to eliminate the errors by which his instrument is affected on each individual occasion of observation. This being generally impracticable, it only remains to make as little use as possible of the supposition of the invariability of the instrument. A clear perception of the necessity for adopting this course has led to the two most extensively-employed principles of modern observation, namely, the principle of the immediate reversal of the instrument, and the principle of the simultaneous measurement of the differences of two nearly equal quantities affected by similar circumstances. It must be acknowledged that the latter of these is in practice preferable to the former. In fact, the instrument *after* reversal, and even by virtue of that operation, is no longer in the same condition as that in which it previously was ; and, notwithstanding that we have thereby eliminated certain sources of error, still it is to be feared that we have at the same time introduced new errors of an unknown character. It is true, indeed, that in the present state of observation an advantage is gained by the process of reversal ; but it is possible that the day may come when this will no longer be so.

No such contingency can, however, occur in the case of the differential method of observation, the advantages of which become more obvious as the observations are more accurate, and as the quantities to be measured are smaller. And yet it is important to remark that, from the nature of this method, the conditions which assure its superiority are not rigorously fulfilled. In each particular case the quantities to be measured are not precisely equal ; they are not affected by precisely the same circumstances, nor are they observed at precisely the same times. It is manifest, therefore, that there exists a determinate limit beyond which the advantages of the method cease to be appreciable. Hence arises the desirableness of obtaining some test by which we might be enabled in any instance to ascertain whether this limit has been attained. This object is frequently effected by a comparison of the probable errors of the same result, as determined by different combinations of the observations. Should any discordance manifest itself in the values of the probable errors as thus deduced, it is thereby demonstrated that the errors of the observations are not merely accidental, but, to a certain extent, regulated by a fixed law. Without knowing this law, it is so far instructive to know that by the combination which assigns the least probable error, the unknown errors of the observations are most effectually diminished. By this means we may generally rely upon having rendered the errors wholly or partially innocuous, although we are unable to ascertain either their nature or their magnitude.

The author contends at great length against the views of Dr.

Wiehmann relative to the existence of a sensible parallax of the star *p*. He is of opinion that such a result is of itself extremely improbable, and he adduces various considerations in support of his assertion. He remarks, for instance, that if the star had the parallax which has been assigned to it by Dr. Wichmann, it ought to have a sensible proper motion. The motion of the solar system in space, which is no longer merely an hypothesis, being directed to a point in the heavens which is distant 77° from the star, almost the whole effect of such a motion ought to exhibit itself in an apparent displacement of the star, to an extent depending on the largeness of the star's parallax. But, since the star has been found to have no sensible apparent motion, it follows that it must have a real motion, the projection of which upon the celestial sphere is equal in magnitude, but opposite in direction, to the effect produced by the motion of the solar system in space. "Now such a coincidence," says the author, "is naturally not impossible, and we should certainly not be warranted in maintaining its improbability in opposition to a result deduced from observation, if we had no reason to doubt the truth of that result. But, on the other hand, to declare that an hypothesis which leads to such a conclusion, is one of a plausible character would be equally untenable."

In the second part of his memoir the author proceeds to consider the disturbing causes which he conceives may have affected the heliometric observations forming the groundwork of Dr. Wichmann's investigation. These he refers to the influences of temperature and flexure. With respect to the former, he is of opinion that the value of the thermometer coefficient as found by Bessel is entitled to reliance as a normal determination of the element; but that the case is quite different when the question refers to any specific measurement. The value of a revolution of the screw, which requires to be introduced into the calculations, is a function of the temperature, not merely of the screw, but also of the material constituting the object-glass; and these two arguments always operate in contrary directions. If the temperature rises, the screw extends, and the value of a revolution is thereby *increased;* on the other hand, an increase of temperature produces an extension of the focal length of the object-glass, and hence arises a *diminution* in the value of a revolution of the screw. According to Bessel the effect produced by those two opposing causes is in favour of the latter; in other words, an *increase* of temperature will tend on the whole to *diminish* the value of a revolution of the screw. The author remarks that the partial compensation which takes place under such circumstances had suggested to him certain ideas on the subject which he does not remember to have found elsewhere. Should these individual effects be nearly equal, and, what is quite possible, considerably greater in each case than their difference, then, in the application of the temperature correction, an error in the thermometric quantity employed as the groundwork of calculation is of much less

consequence than an error in the tacitly-assumed supposition
that the screw and the object-glass have both the *same* tem-
perature. Now this supposition is rarely, if ever, realised;
and it is doubtless exceedingly difficult to ascertain the exact
amount of difference in any individual case. The author de-
scribes an experiment illustrative of the different effects produced
by exposing to the same temperature two thermometers, having
bulbs of *unequal* magnitudes. He contends that a similar
effect must be produced upon the screw and the object-glass
of the heliometer, which are so dissimilar in form and mag-
nitude, and the materials of which possess such unequal powers
of conducting heat. He remarks further, that we ought to
take into consideration not only the difference of temperature of
the *different parts* of the instrument, but also the change
of temperature which occurs *during the time of observation.*
The adjustment of the eye-piece, with respect to the object-
glass, which depends upon the temperature of the latter, is
effected before the commencement of each set of observations.
On the other hand, the temperature of the screw may be con-
sidered as coincident with the indications of the thermometer
corresponding to the time of each individual observation. Sup-
posing each set of observations to occupy an hour, the reading of
the thermometer corresponding to the middle of this interval may
be taken for determining the mean temperature of the screw. The
question then arises, how high shall we estimate the difference
between the temperature of the screw at a given instant, and the
the temperature which determines the focal length of the object-
glass half-an-hour earlier. Supposing this to amount to $\pm 10°$
Fahrenheit, the author finds that the correction to the temperature
coefficient hence arising may amount to $\pm 0''\cdot3$, a quantity which
he shows to be of the same order as the principal part. He main-
tains, therefore, that he was justified in asserting that the question
with respect to the influence of temperature upon the instrument
cannot be considered as exhausted.

From the question of temperature, the author passes to a con-
sideration of the influence of flexure. After a full discussion of
this subject he takes up a series of the observations contained in
Dr. Wichmann's paper, and forms, by means of them, a set of
equations of condition which he solves upon various suppositions
with respect to temperature, flexure, and parallax. The final
result of these various essays may be best stated in his own words.
"It has been demonstrated," says he, "that the observations
under consideration cannot be satisfied by any possible value
of the temperature co-efficient, nor by my formula for flexure,
nor by parallax, nor by all together; other disturbing influences,
regulated by a fixed law, must be in operation. The existence of
such disturbing influences being once established, it is certainly
not admissible to attach to any determination deduced from these
observations a degree of confidence proportionate to the probable
error obtained by means of them. In fact, upon what grounds could

we expect that these unknown disturbances had operated exclusively in the direction of proper motion, and not equally in the direction of parallax, or flexure, &c. For my own part, I am of opinion that to attempt to solve the enigma by the assumption of any other law of flexure, or by a modification of the correction for temperature, were an undertaking which, from the foregoing details, would seem to afford no prospect of success. I must stop here; I must content myself with having shown by this example what I had already established by considerations of a general nature, that in the present condition of our knowledge respecting the influence which an instrument such as the heliometer exercises upon the results of observation, it were utterly impossible to arrive at a reliable conclusion with respect to the cause of the periodic variations amounting to one second of space which the measures of a distance extending to about 59 revolutions of the screw, or 52', exhibit in the course of a year. The undeniable presence of such periodic variations is, however, a fact of such weighty importance that we must not endanger our possession of it by a hasty decision. I here repeat what I have so often and so emphatically stated already, that only a renewed investigation of the subject, carefully prosecuted with reference to the special circumstances of each case under consideration, can lead to a definite result; but we may expect by this means to arrive at a conclusion of a trustworthy nature."

The author proceeds to remark that his criticisms must not be considered as implying that the heliometer is incapable of furnishing any essential contributions to our knowledge of the parallax of the fixed stars. On the contrary, he is of opinion that, in consequence of the large scope which it allows for selecting the stars of comparison, and the extraordinary accuracy with which its adjustment can be specially effected, it is peculiarly adapted for such inquiries. He then shows that, by combining together the results of Schlüter and Wichmann relative to the parallax of A, a value of the latter will be obtained, which is almost entirely free from the error of the instrument, *according to whatever law depending on the distance we may imagine it to operate.* In this way he finds the parallax of the star to be $0''\cdot141$, with a probable error of $0''\cdot013$, supposing the parallaxes of the stars of comparison to be evanescent,—an assumption which, in the present state of our knowledge, he considers to be perfectly justifiable. He remarks that the value of the parallax deduced from a suitable comparison of this result with the corresponding results of Peters and Otto Struve which he also considers to be worthy of confidence, must be considered as one of the most trustworthy determinations connected with the subject; and although it may not chime very accordantly with our notions in the present day respecting the distances and real proper motions of the stars, this circumstance merely indicates to us that we have still much to rectify in respect to those notions.

The author concludes his paper with a vindication of M. Otto

Struve's investigation of the parallax of Argelander's star, which had formed the subject of some critical remarks by Dr. Wichmann.

There is also an Appendix, in which he replies to some objections offered by Dr. Peters to the labours of the same astronomer in No. 865 of the *Astronomische Nachrichten.*

Notes on the Meteor of January 7, 1856.
By Robert James Mann, M.D.

A very brilliant meteor crossed the sky of the south of England during the twilight hour of January 7th, 1856. The writer, happening to be amongst those who were so fortunate as to see it, has thought it well to transmit a few notes on the subject to the Royal Astronomical Society, upon the chance of there being some point that came within the scope of his observation and inquiries which may prove worthy of the notice of such of the fellows of the Society, as particularly interest themselves with meteoric phenomena.

The meteor presented itself at Ventnor, on the south coast of the Isle of Wight, at 5 minutes before 5, local time. The sky was perfectly clear at the instant, excepting for a low cloud-bank, extending from the west pretty well round the horizon. The sun had set nearly three quarters of an hour before; but the twilight was still so strong, that the diminution of daylight was not very obvious. The planet *Jupiter* had passed the meridian an hour and forty minutes, and was only to be discerned after looking for it very closely in the exact position that it occupied. No other star was visible to the unaided eye.

When the meteor appeared, the writer was walking eastwards, along a road open to the sea towards the south. He was suddenly startled by the sense of a great blaze moving along high up in the sky to the right, accompanied with a slight rushing sound.

The flame assumed the form of a disc or ball, very distinctly. It was of an intense whitish-blue hue, and became brighter and bluer as it descended towards the horizon. The ball, too, seemed to assume progressively larger dimensions. When, at its largest development, it appeared to have a diameter of from seven to eight minutes of angular measure. The glare that it cast was as strong as that of a very vivid flash of lightning; but was, of course, more sustained. The blazing ball moved along with about the ordinary speed of the falling stars, when, about seven degrees above the horizon, and a little to the east of the meridian, it plunged into the low cloud-bank already alluded to, having, however lost some of its brightness before doing so, and finally disappeared.

When the meteoric flame had fallen to *within* 35 *degrees of the horizon,* it began to leave a tail of fire behind it, very similar to the trace that is left by a Roman candle on a dark night. The

upper and lower extremity of this tail faded out of sight almost as
soon as it had been observed; but the central portion, through a
range of from fifteen to twenty degrees, remained like a clear
line of incandescent silver burnt into the sky.   At first the line
was almost " mathematical" in its fineness, having no discernible
breadth, and was obviously composed of intensely incandescent
substance; although so fine, it was remarkably bold and distinct
to observation, in consequence of its brilliant whiteness.   In a
few seconds, however, it no less manifestly had acquired width,
and then it went on growing broader, until almost in a couple of
minutes it presented itself as a silvery white spindle-shaped cloud,
standing boldly out from the clear sky.   On this part of the phe-
nomenon the writer's attention was very particularly fixed, and he
is convinced that he saw the luminous trace pass gradually from
the condition of fire, into that of reflective vapour.

When its nebulous character was fully declared, it had at first
the character of a long, slender wand, very nearly straight, and
stretching through an extent of between fifteen and twenty degrees.
The central portion of this wand, however, soon began to bulge
out, until the whole assumed the form of a spindle, tapering at
each extremity.   As it did this, the cloud-composition was more
and more clearly declared, the cloud-substance being gathered
together into subordinate masses, with clear streaks between them,
until the general aspect of the object was of a freckled and then
mottled character.   As this change occurred, the extremities of
the spindle began to bend themselves opposite ways, and the band
itself become sinuous.   After the lapse of two or three more
minutes, the sinuous folds were developed, and the ribbon of cloud
had obtained its full breadth.   The general outline now very nearly
approached to that of a serpent gathering itself up, and rearing
upon its tail.   Soon after this the central fold became more curved
than the rest, and looked like the bend of a horse-shoe.   At this
period the sinuous object was evidently drifting away towards the
south-east, and rising, balloon-like, to a higher elevation as it went,
the middle portion having a more rapid motion than the extremi-
ties.   The sinuous foldings were clearly effects of the different
movements of the atmospheric strata through which the meteor-
track was stretched.   From this time the apparent dimensions of
the cloudy object became smaller and smaller in consequence of
increasing distance, and also its light waned progressively, and its
extremities were shortened by advancing solution.   Sixteen minutes
after the fall of the meteor there remained nothing else visible
in the sky but a very faint streak of inconspicuous, airless cloud,
and in two minutes more was nothing but vacant air.   The
beautiful apparition had vanished entirely.

From a comparison of the observed position of the meteor in
various places, the writer concludes that it fell to the earth within
about ten or twelve miles of the coast of Normandy, and not very
far from Isigny.

*Bemerkungen über die Parallaxe des Argelander'schens Sterns, und
über die Heliometer Beobactungen, von Herrn Dr. Wichmann.*

In an earlier part of this *Notice** is an abstract of an elaborate
memoir by M. Döllen in opposition to the views of Dr. Wichmann
respecting the parallax of the star *Groombridge*, and it is thought
desirable to add an abstract of a recent paper by Dr. Wichmann on
the same subject, which is printed in the *Astronomische Nach-
richten*, Nos. 1010 and 1011. It will be remembered that three
years ago Dr. Wichmann published a remarkable paper in the
Nachrichten on the parallax of this star, in which he discussed the
observations made by himself and M. Schlüter with the heliometer.
The result was curious and startling, inasmuch as it seemed to
show that the more westerly of the stars of comparison had, rela-
tively to the principal star, a parallax amounting to about a second
of space, or, that this star, though of very low magnitude, was
very much nearer to the solar system than the principal star of
which the parallax was sought. Every pains had been taken by
Dr. Wichmann to eliminate all sources of error which might have
caused such an anomalous result, yet after all his pains he could
only arrive at the conclusion that the supposition of the large
parallax of the smaller stars led to fewer difficulties than any other
hypothesis which could be made. Astronomers were naturally led
to view jealously and closely this remarkable result, and, in addi-
tion to the notice taken of it in the annual report of this Society
for February 1853, two profound critiques were written by Dr.
Peters and M. Döllen, the former of which is printed in the
Astronomische Nachrichten, No. 865 ; and the latter (abstracted
in the present *Notice*), in the *Bulletin Physico-Mathém. de l'Aca-
démie de St. Pétersbourg*, tom. xiii.

Dr. Wichmann seems to have hoped to be able to substantiate
or disprove the result referred to by a series of observations of his
own, but being disappointed in this, he at length feels it necessary
to state the views which he entertains of the question at the pre-
sent time.

He remarks that Dr. Peters had proved the uncertainty of the
effect of temperature on the screw of the heliometer, as deduced by
M. Bessel, and had shown that in the observations of the *Pleiades*
constant differences were found in groups of observations made at
different times. M. Döllen had also endeavoured by general con-
siderations to show the uncertainty and want of probability of the
parallax found, and to give another explanation of the unmistake-
able periodical changes exhibited in the measures; yet still the *pos-
sibility* remained, that the star of comparison might really have a
conspicuous parallax, and on this account a *direct* contradiction to
the questionable result by means of new observations was very
desirable. This has been at length effected by the numerous and
excellent measures made by Mr. Johnson with the Oxford helio-

* See 156 *et seq.*

meter in the years 1852 and 1853, published in the fourteenth volume of the *Observations of the Radcliffe Observatory*. While, from the observations made with the Königsberg heliometer, it resulted that the parallax of the western star of comparison was greater by a second than that of the eastern, Mr. Johnson finds, on the contrary, a small excess of parallax for the eastern star. These two contradictory results show satisfactorily that no confidence can be given to the conclusions respecting the parallax of the comparison-stars derived from the sums of the observed distances. A further scrutiny of the Oxford observations has shown Dr. Wichmann that the sums of so great distances can be found by means of the Oxford heliometer with greater accuracy than with the Königsberg instrument, and that consequently the observations made with it are fully entitled to give a decision concerning the questionable difference of parallax of the stars of comparison. " *I have by this means thoroughly convinced myself,*" says Dr. Wichmann, " *that my hypothesis of the remarkable excess of parallax of the western star is no longer tenable ; and further, that the periodical changes of distances exhibited so unmistakeably in the observations have their origin in instrumental causes.* I think it necessary to make this acknowledgment, and I do it so much the more willingly, because my former work might easily give occasion to, and has, in fact, occasioned an erroneous conception of my views. I consequently here repeat expressly that I first took refuge in the hypothesis of the parallax of the star of comparison because all my pains, as well during the observations as the reduction of them, to discover the source of the periodical changes, were fruitless, and I therefore at last held the existence of the parallax of the comparison-star to be at least quite as probable as, nay even more probable than, the existence of periodical errors in the measures, which must certainly appear far less perplexing to one who is not accurately acquainted with the instrument, than to him who, through constant use of it, has learnt to estimate its excellence. The doubts which I entertained concerning the accuracy of my result I could only justify through a feeling of mistrust, and therefore repressed them altogether ; whilst, on the contrary, I brought forward everything that seemed to speak in favour of its probability, in the hope that by this means a more extensive examination of it would be the sooner elicited. I do not believe that by this means the independent judgment of an attentive reader would be prejudiced, since the details of my observations and calculations have been given with sufficient fulness to enable him to separate what is certain from the more doubtful matter, and I leave it, therefore, to the judgment of any reader to determine whether the reproaches made to me by M. Döllen, which besides do not affect the work itself, but the conclusions derived from the computations, are well grounded or not. The tone, little suitable to a scientific object, in which those remarks have been expressed, will in the meanwhile be my excuse for not inserting in this place those remarks, which are utterly irrelevant to the subject.

"If we thus give up the parallax of the comparison-star derived from the distances themselves, there remains as the result of the Königsberg heliometer observations, only that arising from the differences of the measures, namely, that the parallax of *Groombridge* 1830 is greater by $0''\cdot14 \pm 0''\cdot013$ than the mean of the parallaxes of the comparison-stars. This result is not excelled in accuracy by that found by Mr. Johnson, $\pi = 0''\cdot033 \pm 0''\cdot028$, as even the probable error shows, since the Königsberg instrument, with reference to small distances, or in the estimation of the differences of large distances, (at least with the method of observation employed up to this time,) seems to be decidedly superior to the Oxford heliometer. I should not, however, be surprised if, after all, many astronomers will doubt whether the science of observation is sufficiently advanced to *prove* the existence of a parallax of $0''\cdot14$, or whether the influence of one or more unknown causes may not by accident be so nearly coincident with the march of the parallax as to make the calculations indicate a sufficiently great degree of probability."

The candid statement of Dr. Wichmann, which has been given above in his own words, sets at rest the question relative to the parallax of the western star of comparison, which had so greatly excited the interest of astronomers, and at the same time proves that even in the case of so admirable an instrument as the Königsberg heliometer, used with consummate skill, periodical errors may occur in the measurement of large arcs, which may be confounded with parallactic motion of the star observed. Such an investigation as that elicited by Dr. Wichmann's anomalous result is perhaps at this epoch of more value than would have been the successful determination of the parallax of Argelander's star (*Groombridge* 1830); the limits and the sources of all possible instrumental errors will be both for the Königsberg and the Oxford heliometer discovered with greater ease; and the way is opened for more successful attempts at the discovery of very small parallactic motions.

New Planet.

On the 31st of March a new planet was discovered by M. Goldschmidt. The following approximate position of it was obtained by him on the same evening: —

	Paris M.T.	R.A.	Decl.
	h m	h m s	o ,
1856, March 31	10 5	13 13 30	−0 2

The planet discovered by M. Chacornac on the 8th of February has been called *Lætitia.*

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ROYAL ASTRONOMICAL SOCIETY.

MANUEL J. JOHNSON, Esq., President, in the Chair.

New Planet.

On the 22d of May a new planet was discovered by M. Gold-schmidt. It resembles a star of the 11.12th magnitude. The following approximate positions of it were obtained by him:—

	M. T. Paris.	A.R.	Dec.
	h m	h m s	∘ ′
1856, May 22	10 20	10 22 15	+ 10 11
23	11 30	10 23 1	11 15
25	11 10	10 26 10	+ 11 5

The object observed on the 23d of May does not seem to have been the planet.

The planet discovered on the 31st of March by M. Goldschmidt has been called *Harmonia* in commemoration of the peace.

Observations of Double Stars taken at Madras in 1853, 4, 5, and the beginning of 1856. By Eyre B. Powell, Esq.

The equatoreal with which these observations were taken is the same that was used in obtaining the places of Comet II., 1854, recorded in the *Monthly Notices* for June of that year. The object-glass of the telescope is 4 inches in diameter, with a focal length of 63·2 inches. The mounting of the equatoreal is of the German form; and the right ascension and declination circles, which, with their verniers, read respectively to seconds of time and minutes of space, were intended only to find objects or give a rough approximation to their places. The value of a division of the screw-head was found to be 0″·3156. "The instrument," says the author, "is the workmanship of Mr. Simms, and it certainly supports his well-established reputation."

The author then enters into some details with respect to the

mode of observation practised by him. The measures are almost wholly of positions; the author being under the belief that a more powerful instrument and a clock-work movement (which he did not at first possess) were indispensable to the accurate measurement of distances.

The author concludes his introductory remarks with expressing the obligations under which he rests to Captain Jacob for the assistance he kindly afforded him when he began to cultivate an acquaintance with double stars.

The number of stars in the *Catalogue* is 130. Appended to it are a few notes on some of the stars. The following are a few extracts : —

"83. (*α Centauri.*) The observations of this star had extreme care expended upon them. As a general rule, they were taken only when the components were steady and well defined, and the vertical section of the eye was made to coincide with the line of junction of the stars; the position of the observer's head, too, was reversed in the middle of several sets, and many of the angles were measured by daylight.

"The orbit of *α Centauri*, which I laid before the Society in 1854, is undoubtedly erroneous in several particulars; and I believe with Captain Jacob that a thoroughly good set of elements will not be attainable for some years. Still we may arrive at tolerably safe conclusions on certain points. In the first place, it seems likely that the limits within which the period lies are narrower than have been supposed. On drawing the interpolating position and distance curves there is seen to be a necessity for modifying the position and distance for 1831·95 derived from the St. Helena Diff. Decl. = 16″·2, and Diff. R.A. = 1^s·54, and therefore for altering the differences themselves. The curves give P = 216°—40′±, D = 19″·3 ±, and consequently Diff. Decl. = 15·48; but Diff. Decl. = 14″·82 for 1833·0 by Henderson's Cape Observations; hence the annual variation of Diff. Decl. at that time = —·66, a result according with the general tenor of the measures. From this it follows that Diff. Decl. = 16·2 corresponds to 1830·8; and as La Caille's Diff. Decl. = 16·2 belongs to 1752·1 ±, the period comes out 78·7 years. Again, P = 216—40, D = 19·3, afford a Diff. R.A. = 1^s·543, which exactly agrees with Mr. Johnson's result, and harmonises fairly with that of Henderson. Taking the Diff. R.A. of the latter as based upon the greater number of observations, and comparing it with Dunlop's, which equals 1^s·783 for 1826·4, the variation of Diff. R.A. = —0^s·263 for 6·6 years, and Diff. R.A. = 1^s·673 corresponds to 1829·16; but La Caille's Diff. R.A. = 1^s·673 for 1752·1±, therefore we have 77·06 years for the length of the period. Using Mr. Maclear's Diff. Decl. 10″·75 for 1840·0 and the position 223°—10′ suggested by the interpolating curve, the distance for the same epoch = 14″·74. Comparing this with Sir John Herschel's distance = 16·18 for 1837·345, —1·44 equals variation of distance for 2·655 years; and 15″·5 equals the distance for 1838·599. But 15·5± is Maskelyne's distance for

1761·5. Consequently the period from these data is 77·099 years.
In the note on *α Crucis*, reasons are assigned for believing the
space of 79 years to be a superior limit of the period. Combining,
then, that result and the preceding ones, it appears highly probable
that the period lies between 77 and 79 years.

"By cutting up the area swept over by the projected radius
vector since 1826 into various sectors, and examining the latter in
connexion with the position and distance curves, the area for the
interval between 1826·4 and 1855·318, or 28·918 years, is found
to be 57·0954 square seconds, affording an annual sector of 1·974
square seconds. Hence for 77·6 years, a portion of time probably
very near the true value of the period, the area is 153·18 square
seconds, which, therefore, cannot differ much from the space con-
tained within the apparent orbit. The comparison of the several
sectors with the mean areal motion, and the contemporaneous in-
spection of the position and distance curves, lead us to infer that—
the observed distance for 1846·21 is considerably too great—the
one for 1848·01 is somewhat too small—and the sector from
1850·37 to 1854·029 also falls short of the true value, owing, pro-
bably, to slight errors of defect in the distances. A strong suspi-
cion, moreover, is excited that Dunlop's position is too small, and
his distance too great. The comparison of the sectorial areas is of
importance for several reasons; among others, inasmuch as it
seems almost impossible to reconcile the distance for 1846·21 with
that for 1848·01; whence, therefore, it becomes necessary to
decide upon the one that should be preferred as more or less of a
guide in drawing the apparent ellipse. Mr. Hind's orbit, published
in the *Monthly Notices* for January 1855, and one I have lately
calculated, present a curious contrast on this point; but both
agree in attaching an error of nearly a second to one of the dis-
tances. Thus:

Epoch.	P. Obs.	P. Hind.	P. Powell.	D. Obs.	D. Hind.	D. Powell.
	°	°	°	′	′	′
1846·21	232·38	232·92	233·1	10·91	10·34	9·95
1848·01	238·0	237·5	237·95	8·03	8·94	8·55

the sum of the errors of distance being exactly the same for both
orbits. Setting aside the internal evidence of the observations, it
appears far more likely that the distance for 1846·21 is erroneous
to a large extent, than that the distance for 1848·01 should be so.
The former is deduced from only sixteen measures, taken at the
commencement of a series of observations; the latter is the mean
of forty-eight measures, taken after two years' practice. The
axes of the apparent ellipse are still involved in a little doubt;
the Cape meridional observations of 1842 and 1844 favouring a
less excentric ellipse, and agreeing in this respect with the dis-
tance for 1846·21; the measures in general, however, support the
idea of the perspective orbit being peculiarly compressed.

"The position of the periastre is subject to very slight uncer-
tainty; it can differ but little from 32°.

" The measures of 1855 and 6 prove that calculators, myself
included, have considerably antedated the time of periastral pass-
age; that event can scarcely happen before 1862.

" Six orbits, which I computed in 1855, introducing the
latest measures, and in the graphical part using the scale of a
quarter of an inch to a second, and one that I calculated at the
beginning of this year, making use of the positions and distances
just obtained, and drawing the apparent ellipse on the large scale
of half an inch to a second, all agreed in placing the passage in
1862 or 1863 +. At the same time, the observation of Feuillée,
in 1709·5, which belongs to a position of the *comes* past the
periastre, taken with a period of 78 years, will hardly allow of the
passage occurring later than the commencement of 1864.

" To fix with exactitude the node, inclination, excentricity,
and semi-major axis, more especially the last, some additional
years of observing will be required.

" The most satisfactory set of elements, at which I was able
to arrive in 1855, is as follows:—

Apparent Orbit.

$$a = 14''·8 \qquad b = 3''·3 \qquad e = ·95028$$

Greater maximum dist. = 25·4	Pos. for do.	= 207° 30′ ±
Less do. do. = 4·68	Do.	= 3° ±
Greater minimum dist. = 3·73	Do.	= 311° ±
Less do. do. = ·73	Do.	= 94° 30′ ±

Area of Ellipse = 153·5 square seconds.

Real Orbit.

$$\tau = 1862·03$$
$$\varpi = 32° 30′$$
$$\Omega = 14\ 45$$
$$\gamma = 77\ 20$$
$$\lambda = 55\ 35$$
$$e = ·85968$$
$$n = +\ 4·6655$$

Period = 77·16 years
$$a = 20''·8945$$

Area of projected ellipse = $\varpi a^2 \sqrt{1 - e^2} \cos \gamma$ = 153·7 square seconds.

" The measures of 1856 imply that the preceding elements
will require some modifications, as P_c is getting in advance of P_o,
while D_c is beginning to fall short of D_o. Probably, the values of
τ, Ω, γ, e, n and a, will have to undergo changes of about + ·6
year, + 2°, — 2°, — ·06, — 0°·06 and — 2″ respectively. Still
the elements given above are more consonant with the observa-
tions up to 1855·3. The accompanying Table shows the extent
to which an agreement exists between the orbit and observation
up to 1855·3.

Epoch.	P_c $\overset{o}{\ }$ $'$	P_o $\overset{o}{\ }$ $'$	$P_c - P_o$ $'$	D_c $''$	D_o $''$	$D_c - D_o$	Observer.
1826·4	213 50	213 11	+39	21·92	22·45	—·53	Dunlop
1832·16	216 57	216 21	+36	19·19	19·85	—·66	Johnson and Taylor
1833·0	217 28	217 27	+1	18·75	18·67	+·08	Henderson
1834·79	218 42	218 33	+9	17·73	17·4	+·33	Herschel
1837·345	220 44	220 44	0	16·18	16·18	0	Herschel
1840·0	223 19	223 10	+9	14·44	14·74	—·3	{ Maclear Diff. Dec. = 10·74 and P from Curve
1847·09	235 18	235 6	+12	9·26	9·45	—·19	Jacob
1848·01	237 57	238 0	—3	8·55	8·03	+·52	Jacob
1850·37	247 16	247 31	—15	6·75	6·52	+·23	Jacob
1851·05	250 58	251 15	—17	6·25	5·90	+·35	Jacob
1852·561	261 34	262 46	—72	5·21	5·03	+·18	Maclear
1853·049	265 54	267 34	—100	4·91	4·55	+·36	Jacob
1854·029	276 18	276 38	—20	4·39	4·2	+·19	{ P by Jacob & Powell, D. by Jacob
1855·047	289 41	289 3	+38	4·00	…	…	Powell
1855·318	293 34	293 38	—4	3·93	4·07	—·14	Powell

"There is, undoubtedly, error attaching to some one or more of the three positions for 1851, 2, and 3; this is established by the irregularities which the increments of the angular velocities display, and is also indicated by the agreement on this point of all orbits of the star.

"92. *Serpentis.*

Epoch.	Pos.	Observer.
1782·99	227·2	W. Herschel
1821·33	199·22	J. Herschel & South
1829·5	198·4	J. Herschel
1842·35	196·2	Smyth
1853·303	193·07	Powell

"This star is undoubtedly in motion; but, as is remarked in the *Cycle*, its retrogradation is slower than was at first imagined. Perhaps Sir W. Herschel's angle should be 207·2; if so, all the observations would harmonise pretty well.

"99. *σ Coronæ Borealis.* Captain Jacob's orbit for this star, contained in the *Monthly Notices* for April 1855, and my own, published in the *Monthly Notices* for January 1855, approximate to each other in most respects; the chief differences are in the excentricity and mean motion. In my computations I found that the earlier measures favour an increase, and the later a decrease of the mean motion; since the more recent observations are probably the more exact, I am inclined to think that my periodic time is too short, instead of too long, as Captain Jacob's results imply. However, the lapse of some years is requisite to settle this point.

"108. *70 Ophiuchi.* I have devoted considerable attention to this binary, as well in investigating its orbit as in measuring its

position angles ; and while I cannot resist the evidence borne by the results of such accurate observers as the Rev. Mr. Dawes, Captain Jacob, and Mr. Fletcher, with regard to the diminution of the distance, I am unable to reconcile the comparatively great change in that element with the companion's slow motion in angle. I have attempted, but with no good success, to improve upon the orbits I arrived at in 1854, and which appeared in the *Monthly Notices* for December of that year. Although a fresh set of elements, derived from the solution of equations of condition, afforded a better series of distances, the corresponding angular errors were much increased.

"As remarks have been made, assuming that the distance, according to my orbit of 1854, is still on the increase, I may be allowed to mention that such is not the case ; the maximum was reached about two years ago. •

"126. *Cycle* 836. This star is well worthy of attention, the *comes* having described an angle of about 6° in a retrograde direction since 1834·79. The distance also has undergone a considerable decrease.

Epoch.	Pos.	Dist.	Observer.
1824·8	274·07	7·98	South.
1834·79	272·1	7·5	Smyth.
1855·873	266·12	6·23	Powell.

"As A's proper motion would produce a variation in the position angle contrary to that which has taken place, and B has had no proper motion assigned to it, there exists a strong probability of the physical connexion of the pair."

Observed Right Ascensions and North Polar Distances of recently discovered small Planets obtained from Meridian Observations at the Royal Observatory, Greenwich.

Victoria.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1856, April 4	8 36 9·3	9 29 30·49	86 55 54·39
7	8 24 0·3	9 29 9·25	86 40 34·32

Euphrosyne.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1856, April 4	9 15 36·3	10 9 3·99	47 32 39·78
12	8 42 43·0	10 7 37·73	49 21 30·76

Egeria.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, April 4 9 21 52·6	10 15 21·39	56 45 17·29
15 8 37 24·1	10 14 7·63	58 40 43·89
21 8 14 41·3	10 15 0·44	59 49 58·58

Lætitia.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, April 4 9 51 2·1	10 44 35·61	78 40 17·24
12 9 16 45·9	10 41 46·20	78 8 52·21
16 9 0 7·8	10 40 51·57	77 56 58·07
19 8 47 52·5	10 40 23·93	78 49 47·13
21 8 39 48·6	10 40 11·83	77 45 45·62

Thetis.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, April 4 12 33 26·8	13 27 27·05	89 1 38·45
10 12 4 44·2	13 22 18·97	88 21 33·90
12 11 55 8·3	13 20 34·68	88 9 43·42
21 11 12 8·0	13 12 56·29	87 21 39·12
22 11 7 23·8	13 12 7·81	87 17 9·31
28 10 39 (10),		86 54 46·86

Harmonia.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, April 12 11 36 30·7	13 1 53·96	88 53 31·96
19 11 2 33·9	12 55 27·45	88 22 48·80
21 10 52 58·8	12 53 43·95	88 15 31·13
22 10 48 12·8	12 52 53·67	88 12 9·01
24 10 38 43·6	12 51 16·09	88 5 52·90

The N.P.D.'s are corrected for refraction and parallax.

Note on Variable Stars. By A. D. Wackerbarth, Esq.

" Suppose a nebula, such as that from which the earth, sun, and planets, are supposed to have arisen, existing in space. Such a nebula would probably be composed of elements more or less the same as those whereof our own planet is formed. Some, indeed, of the latter we might suppose wanting, and others present which we possess not here; but on the whole, let us suppose that the chief elements of the earth are found in our nebula, which would thus form an immense spheroïd of nebulous matter revolving round its own minor axis; or rather, if the matter were not quite homogeneously distributed, on an axis passing through its centre of gravity. We may suppose, or not, as we please, that this nebula has a nucleus, as many nebulæ appear to have, and many not to have, any such portion; but in the former case we must suppose some little difference in the constitution of the particles towards the centre or position of the said nucleus. Our nebula, thus composed, may wander a longer or shorter period in space peacefully; but now let us suppose a disturbance such as that which broke up the nebulosity of the mass which forms the planetary system, and condensed it into separate globes. Such disturbance might come from without or from within; there are forces in nature to account for either. We have supposed all or many of the elements present; but in a nebulous form they would be in a finely-divided state, and many of them, perhaps all the baser metals, have such affinity to oxygen, as when, in that state, to take fire on coming in contact therewith; so that any cause, which would bring them and the oxygen into contact, would cause fearful explosions, and set the whole nebula in ignition, condensing it into burning masses, each by the violent explosions casting out smaller fused and burning masses into space, to revolve as burning globes around itself or the centre of gravity of the whole. But what would be the condition of one of these fused and blazing masses? Hydrogen is present as well as oxygen; and ignition must therefore immediately cause the production of water. An ocean is then poured down on the incandescent globe, to be cast up again, as steam, into a damp atmosphere formed by the nitrogen and the watery vapour. Here it cools, and is again poured down in torrents on the glowing mass; and this process must continue until the globe has sufficiently cooled for the water to be able to rest upon its surface. Now I imagine that the appearance of a globe in such a state, viewed at a stellar distance, must be variable; that, when the water is, in the form of steam, driven up into the atmosphere, the burning mass must glow with greater brilliancy; but when, the steam condensing, a boiling ocean is poured upon it, the violence of the conflagration must for a time abate, and thus the object assume a less brilliant appearance until the fire has succeeded in reconverting the water into steam, and driving it up again into the atmosphere.

" Another circumstance may be mentioned as possible, namely,

some bodies may be at present in some parts already to a considerable degree cooled; at least sufficiently to be, at those parts, extinct, while other parts of them are yet fused and burning. Would not the revolution of such a body present the phenomena of a variable star ?

" *Uppsala*, 1856, *Apr.* 22."

Description of an Observatory erected at Upper Tulse Hill.
By William Huggins, Esq.

" A short account of an Observatory lately erected by myself at Upper Tulse Hill will not, I think, be considered altogether void of interest, as the building combines, in an unusual manner, the several important advantages of perfect convenience of access, uninterrupted view of the horizon, and freedom from tremor.

" The building, as will be seen by the model, is raised upon columns to a height of sixteen feet above the ground, and is connected by an enclosed passage with the upper story of the cottage.

" It becomes thus, for all purposes of convenience and access, a part of the house; while its elevated position prevents the view of the heavens from being obstructed, as would otherwise be the case, by the cottage and neighbouring trees.

" The stability of the instruments is provided for by two massive, pyramidical columns of brickwork built in cement, and resting upon deep and broad foundations of concrete. These columns pass up through the floor of the building, with which they are wholly unconnected, to a proper height for the reception of the instruments. The iron columns, upon which the building is supported, repose likewise upon solid concrete foundations.

" The building, which is 18 feet long by 12 feet wide, is formed of a strong framework of wood. This is covered externally with plates of corrugated iron. To the inside of the framework a double boarding, with felt placed between, is nailed, and the whole of the interior is hung with varnished oak paper. The dome, 12 feet in diameter, is hemispherical, and rotates easily on three iron balls, running in channels of iron plate. The shutter of the dome, 18 inches wide, extends through slightly more than a quadrant; it runs upon rollers, on parallel ridges, placed outside the dome. The ridges are continued over from horizon to horizon, and the shutter travels over and back again by means of two lines of wire-rope attached to the axle of a small windlass, fixed on the inner side of the curb of the dome. The dome itself consists of a circular wooden curb, bearing a light frame-work of iron, covered over with thin sheet-zinc. The dome is lined with felt and painted oil-cloth. The transit-shutters are arranged in the usual manner.

" The following instruments are at present in the Observatory.

" An equatoreal, by Dollond, 5 inches aperture, and 5 feet focal length ; circles 18 inches diameter.

" A transit-circle, by the late Thomas Jones. The telescope has a focal length of 45 inches, and an aperture of 3·25 inches. The circle is 18 inches diameter, with divisions on silver to 5′. The verniers read to 3″.

" The clock is an excellent one, by the late T. Arnold."

Observations of Planets made by Mr. James Breen with the Northumberland Equatoreal at the Cambridge Observatory.

(*Communicated by Professor Challis.*)

Atalanta.

	Greenwich M.T.	R.A.	Par. Corr. ×Δ	N.P.D.	Par. Corr. ×Δ	No. of Comp.	Star.
	h m s	h m s	s	° ′ ″	″		
1855, Oct. 24	9 30 29·9	22 49 5·39	+0·080	94 54 52·90	−7·16	4	a
	10 48 54·5	22 49 4·30	+0·191	94 54 20·72	−7·10	2	a
Nov. 3	7 42 21·4	22 48 0·11	−0·023	93 11 31·24	−7·03	4	b
Dec. 20	7 27 1·1	23 24 40·15	+0·174			12	c
	7 28 25·8			83 36 16·86	−6·20	6	c
21	7 17 5·7	23 26 0·57	+0·164	83 22 55·83	−6·16	8	c
22	7 0 6·7	23 27 21·53	+0·144	83 9 19·27	−6·13	8	d
1856, Jan. 25	7 36 6·1	0 22 46·88	+0·289	75 4 35·31	−5·70	8	e

Assumed Mean Places of the Stars.

	Mean R.A. 1855·0.	Mean N.P.D. 1855·0.	Catalogue.
	h m s	° ′ ″	
a	22 46 40·15	95 12 14·81	Bessel XXII. 971
b	22 46 2·83	93 4 27·71	— XXII. 961
c	23 24 50·44	83 42 45·65	— XXIII. 524
d	23 26 46·33	82 58 2·77	— XXIII. 538
	Mean R.A. 1856·0.	Mean N.P.D. 1856·0.	
e	0 23 18·69	74 45 33·71	B.A.C. 116

The assumed R.A. and N.P.D. were derived from the Catalogues named, with the exception of the R.A. of Bessel XXII. 961, which was obtained from two Cambridge Observations in 1850, which gave the R.A. greater than that in Weisse's Catalogue by 1ˢ·63.

Harmonia.

	Greenwich M.T.	R.A.	Par. Corr. ×Δ	N.P.D.	Par. Corr. ×Δ	No. of Comp.	Sta
	h m s	h m s	s	° ′ ″	″		
1856, Apr. 24	13 8 39·6	12 51 11·17	+0·215	88 5 45·97	−6·61	6	a
28	12 27 45·8	12 48 11·96	+0·187	87 55 37·44	−6·58	4	a
30	9 52 31·6	12 46 53·91	−0·027	87 51 52·30	−6·55	2	a
	10 9 54·4	12 46 53·22 (?)	−0·001	87 51 58·63 (?)	−6·55	7	b
May 20	12 25 11·7	12 38 50·71	+0·290	87 56 53·42	−6·64	8	c

Assumed Mean Places of the Stars.

	Mean R.A. 1856·0.	Mean N.P.D. 1856·0.	Catalogue.
	h m s	o ′ ″	
a	12 50 6·60	87 47 31·59	Bessel XII. 854
b	12 47 48·35 (?)	87 44 1·45 (?)	Not in Catalogues
c	12 37 57·87	88 9 11·11	Bessel XII. 638

The assumed place of the star *b* depends on a single equatoreal comparison
with *a*, and therefore requires correction by further Observations.

Lætitia.

	Greenwich M.T.	R.A.	Par. Corr. × Δ	N.P.D.	Par. Corr. × Δ	No. of Comp.
	h m s	h m s	s	o ′ ″		
856, May 22	11 38 29·6	10 47 34·59	+0·343	77 58 9·80	6·29	10

The star of comparison was Bessel X. 833. The assumed mean place 1856·0
is, R.A. = 10^h 45^m 52^s·64, and N.P.D. = 77° 40′ 41″·82.

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*Astronomical and Meteorological Observations made at the Rad-
cliffe Observatory, Oxford, in the year* 1854, *under the Super-
intendence of Manuel J. Johnson, M.A., Radcliffe Observer.*
Vol. XV. Oxford, 1856.

Besides the usual observations with the transit instrument and
meridian circle, this volume contains the commencement of the
Catalogue of Stars remarkable for physical or other peculiarities,
which Mr. Johnson purposes to construct pending the preparation
of the *Circumpolar Catalogue* for the press, and to which allusion
was made in the last Report of the Council. The number of stars
is 835, all of which were observed in 1854. There is also a
catalogue of 164 stars within 6° of the pole, which Mr. Johnson
contemplates extending from time to time as far as circumstances
will admit. The heliometer has been employed exclusively in
observations for determining parallax. *Arcturus, Castor,* and
α Lyræ, have been the principal objects of investigation; but as
the series of observations had not been in any instance completed,
it was considered desirable to postpone their publication. The
volume contains a catalogue of variable stars which Mr. Pogson,
one of the assistants at the Observatory, drew up originally for
his own private use during his leisure hours. This Catalogue
has also been published in a separate pamphlet; a brief notice
of its contents will be found at p. 185.

The remaining part of the volume is devoted to meteorological
observations, and to the results of discussions of such obser-
vations. We find in this section the commencement of a regular
system of registration by photography, which is now in full
operation at the Radcliffe Observatory.

Note de M. Jean Plana sur les pages 60 *et* 61 *du* 1er *volume de sa*
    Théorie du Mouvement de la Lune.

This memoir of M. Plana contains an admission and correction
of an error in his Lunar Theory, relative to the secular acceleration
of the moon's mean motion, of which it is impossible to over-esti-
mate the importance. Mr. Adams had drawn attention both to
the inadequacy of La Place's theory respecting this secular in-
equality, and to the serious correction which must be applied to
the numerical coefficient of m^4 in its expression, in a remarkable
paper, printed in the *Philosophical Transactions* for 1853, Part III.,
of which a good extract will be found in the *Proceedings of the Royal
Society* for that year. His attention had been drawn to this subject
while endeavouring to supply an omission in the theory of the
moon given by Poutécoulant in his *Théorie Analytique.* In this
work, though the author follows Sir J. Lubbock's method of ex-
pressing the co-ordinates of the moon directly in terms of the time,
yet he unfortunately adopts Plana's results without examination.
Mr. Adams, in performing the calculations requisite to complete
the defective part of the theory, was surprised to find that the
second term of the secular acceleration thus obtained not only
differed considerably in magnitude from the corresponding term
given by Plana, but was even of a different sign. Referring then
to the *Proceedings of the Royal Society,* page 321, for a sketch of
Mr. Adams' processes, it will be sufficient here to give his result,
compared with that of Plana. This result is only carried to the
second term of the function expressing the acceleration, that is, to
the fourth power of m (the ratio of the mean motions of the sun
and moon), since it was the author's object to exhibit in its simplest
form the error which he had discovered. but he at the same time
expressed his intention of carrying his approximation still farther,
so as to obtain the coefficients with an accuracy sufficient for the
calculation of remote eclipses.

The expression obtained by Mr. Adams for the first two terms
of the acceleration, was :—

$$-\left(\frac{3}{2}m^2 - \frac{3771}{64}m^4\right) \int(e'^2 - E'^2)\,n\,d\,t$$

while, according to Plana's *Théorie de la Lune,* the corresponding
expression is,

$$-\left(\frac{3}{2}m^2 - \frac{2187}{128}m^4\right) \int(e'^2 - E'^2)\,n\,d\,t$$

The difference of the two expressions is,

$$\frac{5355}{128}m^4 \int(e'^2 - E'^2)\,n\,d\,t$$

and the numerical value of the correction is,

$$- 1''\cdot66\left(\frac{t}{100}\right)^2 .$$

or — 1″·66 multiplied by the square of the number of centuries from a fixed epoch.

In the year 1854, M. Plana published in the *Turin Memoirs* a short note in which he discusses the result arrived at by Mr. Adams. After indicating the pages in his *Théorie*, in which will be found the details of the calculations for the completion of the second term of the function which expresses the secular acceleration, he remarks, " I cannot establish a complete comparison between the result of Mr. Adams and my own, without again going through all the details ; which would require a very painful labour, from which I think I may hold myself excused, by the care with which I have published all the intermediate results. By means of them we can always keep before the eyes the proof of the definitive results, upon which we ordinarily concentrate all the importance which they deserve, on the supposition that they are incontestable, whatever may be the complication inherent in their deduction." After a few words of explanation respecting his method, tending to prove that no essential term depending upon the secular varia- tions of the orbit of the earth has been omitted, he concludes his note by recommending the part of his own theory under dis- cussion " to the judgment of all those who would be willing to submit it to a profound and severe examination."

It appears, therefore, that at the time of writing this note, M. Plana had no suspicion of anything defective either in his own processes or in the theory of La Place, nor has any astronomer, from that time to the present, followed his recommendation "to study profoundly the complicated processes" on which his result for the secular acceleration is based. Fortunately, however, the author has recently found occasion to do this himself, and has not only discovered the source of the error, which occasions the discrepancy between his own expression and that of Mr. Adams, but with most commendable activity and energy, has thoroughly traced the effect of it in every term where it produces any sensible effect, and gives the result of his researches in a memoir, which is the chief subject of this article. The chief point for observation is that the correctness of Mr. Adams' expression for the secular acceleration to the second term of the function is fully verified, and thus the necessity is recognised of applying a correction to the mean motion of the moon, which will interfere materially with all recent chronological speculations, and make a recomputation of the elements of all ancient eclipses imperative.

As usual, the source of the error was very simple and obvious, though, singularly enough, it has escaped the notice of all astro- nomers. M. Plana's account of it is as follows. In treating of the secular variations of the elements of the solar orbit, he has occasion to discuss the integral expressed by the equation,

$$x = B \int \epsilon'^m \sin (k \, \imath + \beta - n \, \tau') \, d \, \imath$$

which, he says, comprises all the cases of integration which can

present themselves in the research of the co-ordinates of the moon. By means of processes, which it is not necessary to give in detail, he finally arrives at the following equation expressing the value of x,—

$$x = -\frac{B}{k} \varepsilon'^m \cos(k\, \nu + \beta - n\, \tau') - \frac{B n\, d\, \tau'}{k^2\, d\, \nu} \varepsilon'^m \cos(k\, \nu + \beta - n\, \tau')$$

$$+\frac{B m}{k^2} \varepsilon'^{m-1} \frac{d\, \varepsilon'}{d\, \nu} \sin(k\, \nu + \beta - n\, \tau')$$

but by a strange typographical error the last of the three terms given above was omitted in the text near the end of page 61 of the 1st volume of the *Théorie de la Lune*. That this error was purely typographical is evident from the words which follow, namely, " l'on peut réunir ces trois termes en un seul, et poser

$$x = -\frac{B \varepsilon'^m}{k - n\frac{d\, \tau'}{d\, \nu}} \cos\left\{ k\, \nu + \beta - n\, \tau' + \left(k - n\frac{d\, \tau'}{d\, \nu}\right) \frac{m}{\varepsilon'\, k^2} \cdot \frac{d\, \varepsilon'}{d\, \nu} \right\}$$

" Thus," he says, " it happened, that, in composing the note of the 17th December, 1854, I did not pay attention to the omission of the term

$$\frac{B m}{k^2} \varepsilon'^{m-1} \frac{d\, \varepsilon'}{d\, \nu} \sin(k\, \nu + \beta - n\, \tau')$$

which should form part of the value of x which immediately precedes the last. In consequence of this omission I saw only the term multiplied by

$$\varepsilon'^m \frac{d\, \varepsilon'}{d\, \nu}$$

and I considered that the one multiplied by

$$m\, \varepsilon'^{m-1} \frac{d\, \varepsilon'}{d\, \nu}$$

was destroyed by others. To this circumstance must be attributed my method of recognising (*ma manière de voir*) the non-existence of the periodical terms multiplied by $\frac{d\, \varepsilon'}{d\, \nu}$ in the expressions of δu and $\delta n t$. As soon as I had re-established, by performing again the calculations of page 61, the true trinomial in the place of the monomial equal to the value of x, I saw that the integration will introduce secular terms into the integral, namely, products of the form

$$x\, B'\, \varepsilon'^m \sin(k\, \nu + \beta - n\, \tau')\, d\, \nu :$$

for there will be the term

$$\int \frac{B B'm}{k^2} \varepsilon'^{2m-1} \frac{d\, \varepsilon'}{d\, \nu} \sin^2(k\, \nu + \beta - n\, \tau')\, d\, \nu$$

$$= \int \frac{B B'm}{2\, k^2} \varepsilon'^{2m-1}\, d\, \varepsilon' = \frac{B B'}{4\, k^2} \varepsilon'^{2m}.$$

Hence the existence of terms of the form

$$G\,\varepsilon'^{2m-1}\,\frac{d\,\varepsilon'}{d\,\nu}\,(\sin k\,\nu + \beta - n\,\varpi')$$

in the expression of δu is incontestable. Their form is similar to that of the term $Q \sin(i\,\nu + \beta)$ of which La Place has remarked the existence at page 214 of the third volume of the *Mécanique Céleste.*"

M. Plana then proceeds to investigate the secular terms which will arise from the omitted term of his trinomial; and, after an investigation of considerable length, arrives at precisely the same result as Mr. Adams had obtained by a different process.

He then proceeds with the investigation of three other terms of the same order, arising from the omitted term of the trinomial.

The first of these, which is to replace the corresponding term in the secular equation of the mean motion at page 485 of the first volume of the *Théorie de la Lune*, is

$$-\frac{99}{128}\,m^2\,e^2\int(\varepsilon'^2 - \mathrm{E}'^2)\,d\,\nu$$

the term, as it stands at the place cited, being

$$+\frac{1461}{128}\,m^2\,e^2\int(\varepsilon'^2 - \mathrm{E}'^2)\,d\,\nu$$

The second term, which must replace the corresponding one on the same page as before, is

$$+\frac{447}{128}\,m^2\,\gamma^2\int(\varepsilon'^2 - \mathrm{E}'^2)\,d\,\nu$$

instead of

$$+\frac{525}{128}\,m^2\,\gamma^2\int(\varepsilon'^2 - \mathrm{E}'^2)\,d\,\nu$$

Finally, the third term, which must replace the corresponding one in the *Théorie* (same page as before), is

$$-\frac{75}{32}\,\nu^4\int(\varepsilon'^2 - \mathrm{E}'^2)\,d\,\nu$$

instead of

$$+\frac{75}{32}\,b^4\int(\varepsilon'^2 - \mathrm{E}'^2)\,d\,\nu$$

Hence the correct expression for the secular equation of the mean motion of the moon to terms of the fourth order, is as follows:—

$$\int\zeta\,d\,\nu = \left(\frac{3}{2}m^2 - \frac{3771}{64}m^4 - \frac{99}{128}m^2\,e^2 + \frac{447}{128}m^2\,\gamma^2 - \frac{75}{32}b^4\right)\int(\varepsilon'^2 - \mathrm{E}'^2)\,d\,\nu$$

$$+\frac{15}{8}m^2\int(\iota'^4 - \mathrm{E}'^4)\,d\,\nu\,*$$

* In Plana's Memoir, this term has for its coefficient $\frac{15}{2}$, which is presumed to be error.

instead of

$$\int \zeta \, d\nu = \left(\frac{3}{2} m^2 - \frac{2187}{128} m^4 + \frac{1461}{128} m^2 e^2 + \frac{525}{128} m^2 \gamma^2 + \frac{75}{32} b^4 \right) \int (\varepsilon'^2 - \mathrm{E}'^2) \, d\nu$$

$$+ \frac{15}{8} m^2 \int (\varepsilon'^4 - \mathrm{E}'^4) \, d\nu ,$$

as it stands in the *Théorie*, vol. ii. p. 852.

With regard to these results M. Plana observes :—

" The calculation of the last three terms requires considerations more delicate than those for the term multiplied by m^4. The agreement which I now obtain with respect to the term multiplied by m^4, proves the correctness of my preceding calculations ; otherwise it would have been impossible to cause the discordance to disappear by this *simple addition* of the terms, which I have calculated in this note, by the assistance of my formula [1] resulting from the analysis which I have explained in pages 60 and 61 of the first volume of my work."

M. Plana, after obtaining by analysis the periodical terms multiplied by $\dfrac{d \, \iota}{d \, \nu}$ in the expression for the mean longitude, and showing that they are insensible, concludes with the following remarks :—

" I reserve for another time the calculation of the terms of the fifth, sixth, and seventh orders, which ought to be added to those which I have already calculated, in order to complete, to the last order at least, the co-efficient of the secular equation of the mean motion of the moon. The formulæ which I have established in this note are important for directing the choice of arguments and of the terms of their co-efficients, which must be considered in order to comprise in this definitive result all the quantities of the same order. For the present, it is sufficient for me to have completed the calculation of terms of the fourth order, indicating at the same time the sources from which may be derived the following terms by the aid of the different functions which are developed in my work. This greater precision is necessary that we may be in a condition to verify, not only by observations of eclipses, but also by those of the *libration* separated by intervals of several thousand years, that the secular equation in question is communicated by the attraction of the earth to the mean motion of rotation of the lunar spheroid with such uniformity, that it will always be impossible to see the hemisphere opposite to that which the moon now presents to us."

Notice of the Observatory of the Collegio Romano.

The Council have been favoured, by the courtesy of Mr. G. Rennie, with a communication of some particulars of the present state of the Observatory at Rome, which he obtained from Professor Secchi during a recent visit to that city. The original account in Italian has been translated by the ladies of Mr. Rennie's family, and is accompanied by two plans, one a horizontal, and the other a vertical section of the building through the observing-rooms, from which a very good idea of the establishment may be procured. The account, as transmitted to the Council, being too long for insertion in the *Monthly Notices,* has been condensed into the following form.

The Jesuit College is situated immediately at the east extremity of the church of St. Ignatius. In the original design of this church, it was intended that a cupola of $58\frac{1}{2}$ feet (Engl.) in diameter should crown the east end of the church; and pilasters of solid masonry, and of a section of about 520 square feet, were introduced into the structure for its support. This cupola, having never been built, owing to change of plans consequent on the premature death of Cardinal Ludovici, advantage has been taken of the solid foundation intended for it, to erect thereon, at about the level of the roof, piers and observing-rooms for the astronomical instruments, possessing all that is necessary, in the way of elevation, horizon, and convenience of access from the College.

The principal observing-rooms are the meridian circle-room and the dome for the large equatoreal. The former is of elliptic section, measuring about 23 feet in length, north and south, by 16 in width, and contains three fixed instruments and two clocks, one going in sidereal, the other mean time. The three instruments mentioned are placed on three piers, ranged north and south, under the roof-opening, which is common to them all. Near to the south shutter is a transit instrument of 4 feet focal length, by Reichenbach, mounted on a cast-iron stand, bedded in masonry; and so contrived that the pier may be turned round through 90 degrees, if desired, for observation in the prime vertical, without losing, after refixing, its character of a stable support. In the centre is placed the meridian-circle, by Ertel, for a description of which the reader is referred to the Memoirs of the Observatory. The focal length of the telescope is 5 feet, and the diameter of the circle $27\frac{1}{2}$ inches. Since the former description was written, some additions have been made to the micrometrical parts of the wire-frames; and provision has been made for illuminating the wires in a dark field at night. The graduated circle and the reading apparatus is cased in with a covering of bright metal to ensure equable and slow distribution of varied temperature. In this case are four small shutters in the direction of the illuminating rays, which are opened in succession when a reading has to be

B

taken, and are then closed again. The additional precaution has also been taken of so distributing the four windows, that the light and heat coming from without may fall about equally on all parts of the instrument. The third instrument, occupying the north end of the room, is a collimating telescope for the Ertel circle, which is partly used for checking the position of the meridian instrument at times when observations of the polar stars have not been obtained. As the stability of the collimator, however, in itself could no more be relied on than that of the meridian instrument, there is joined to the collimator a second telescope, directed constantly on an object-glass, and mark distant (northwards, probably) about 550 yards. It is assumed that the collimator and its companion move together; and that the changes of position of the one may be attributed to the other.

Professor Secchi remarks, that in a recent examination the iron trough used for the mercurial horizon was found to be strongly magnetic, and that, in consequence, he thinks, the glass troughs, formerly in use, to be preferable.

The equatoreal room is circular, and 25 feet in diameter, surmounted by a movable roof, in the form of a cylinder, covered in with a segment of a sphere. The construction of this dome is described at much length; but, possessing no peculiar feature of novelty, may be omitted. Its shutters are conveniently opened by winch-work and chains, and closed again by merely reversing the movement. An excellent horizon for the equatoreal is obtained by raising the room, so that the centre of motion of the telescope is above the level of the roof of the meridian room, while the meridian of the latter is not interfered with by the dome in consequence of the two not being quite north and south. The equatoreal, which is the first and largest of its kind in Italy, is precisely similar to the one at Pulkowa, excepting in size. The mounting appears to be very similar to that of the Oxford heliometer in this country, every important bearing being provided with friction-wheels and counterpoises, after the German fashion. The whole is carried round by a driving-clock, of the excellent performance of which Professor Secchi speaks in terms of great praise. The rate of the regulating part of this movement is controlled by the friction of two small brass balls against the sides of a conical box, in a way which many readers of the *Monthly Notices* have probably themselves witnessed. The object-glass of the equatoreal has an effective aperture of 9 French inches, and is not found to be in any degree improved in performance by the application of diaphragms to diminish the full opening. The focal length is 14·2 English feet. The telescope is provided with various positive and negative eyepieces, ranging from about 100 to 1000 in magnifying power. In the observation of very faint objects Professor Secchi has not been entirely satisfied with the means for illuminating the wire-frame, the observer always, more or less, finding himself annoyed by stray light about the room; and, after various experiments, expresses his intention of applying internal

illuminations by rendering a piece of platinum wire incandescent by galvanic current.

Besides the above rooms and instruments, there is a vacant turret on the roof of the church, which is used on the site for a small telescope by Cauchoix, employed for open-air observations; and within the Observatory are stored various theodolites, toises, surveying instruments, quadrants, and chronometers.

There is a complete collection of instruments in use for meteorological observations; and the precaution is taken of always having a duplicate of each in store to prevent interruption of the records by the derangement or breakage of thermometers. A zinc-ball is also in regular use; for which the mean-time clock in the transit-room is used, and by which the instant of noon is daily signalled to the city.

In the Library of the Observatory are kept all the books and maps in frequent use; and generally the more modern works belonging to the Observatory, while the more ancient works, forming a rich collection, are preserved near at hand in the Library of the College.

A very beautiful copy of a drawing of the large lunar crater, *Copernicus*, executed with great labour and success, was also presented to the Society at the same time by Professor Secchi, and greatly admired by those members of the Society, who were present on the occasion when it was laid on the table.

Catalogue of 53 Variable Stars, with Notes. By Norman Pogson, Assistant at the Radcliffe Observatory. (*From* Vol. XV. *of the Radcliffe Observations.*)

It has been stated at p. 177, that this Catalogue was prepared by Mr. Pogson during his leisure hours, and was originally intended solely for his own private use. So complete a compendium of the results which have been established in this interesting branch of astronomy, cannot fail to be acceptable to astronomers generally. It contains the mean place of each star for the year 1860, with the annual variations in right ascension and north polar distance; the magnitudes at maxima and minima; the mean period in those cases wherein such has been ascertained; the discoverer of variability; and, lastly, the date of discovery. The notes appended to the Catalogue contain a variety of useful details. Of the variable stars which have been discovered by Mr. Pogson himself, one of the most interesting is the star 24 *Ceph.* (*Hev.*), which has been confounded by Lalande and other astronomers with λ *Ursæ Minoris*, but which Mr. Pogson has shown to be in reality identical with No. 3402 of the Astronomer Royal's edition of Groombridge's Catalogue. We extract the following from Mr. Pogson's Notes :—

" 36. *S. Ophiuchi.* This star was first seen at the Radcliffe Observatory, as a 10th mag. on 1854, May 6. In June 1853,

I had carefully constructed and revised this portion of a manu-
script chart, taking in all stars down to the 12th inclusive, and
am sure, that if visible at all, at that time it must have been too
faint for insertion in my chart.   The third night after its recog-
nition it was very little under a 9th, after which it rapidly faded
away, no trace remaining by the middle of June.   It continued
invisible through July, August, and September, of that year.   In
April and May 1855 it was still invisible, and therefore less than
13·5, my limit of vision with the equatoreal of 7·2 inches aperture.
On June 9 it reappeared, strictly as a *minimum visibile*.   It must
have attained a maximum some time in August ; for on Sep-
tember 10 and 12 it had again diminished to the 11th.   On 1856,
March 7 and 11, it was 9·3.   Hence the period is probably about
7 months.   The colour when brightest was bluish white ; no
redness was remarked at any time.

"Near *S. Ophiuchi* we find one of the most remarkable
vacuities in this hemisphere—an elliptic space of about 65′ length
in the direction of R.A. and 40′ width, in which there exists *no*
star larger than the 13th magnitude.   The centre of this curious
blank is situated in R.A. 16^h $18\frac{1}{4}^m$, N.P.D. 160° 40′, and it is
impossible to turn a large telescope in that direction, and if I
may so express it, view such black darkness, without a feeling
that we are here searching into the remote regions of space, far
beyond the limits of our own sidereal stratum."

"42. 2896 *Gr.*   This star, which is in the same field of view
with *θ Cygni*, was observed 4 times by Groombridge in 1811,
and recorded by him as of 7·5 mag.   It was frequently looked
for with the meridian instruments of the Radcliffe Observatory
between 1841 and 1845, but not seen.   In Argelander's Zone 21,
observed on August 16, 1841, we find the two bright stars pre-
ceding, but not this one.   On August 27, 1852, having set for its
place with the transit instrument, I was surprised to find it bright,
and easily observable with full illumination, certainly not much
under the 7th mag.   I have since obtained upwards of 120
comparisons on different nights.   A graphic projection furnished
the period given in the table, which, though only approximate,
agrees very well with the dates of Groombridge's observations.
The curve of variation is remarkably steady in its march.   The
increase from 13·5 mag. to maximum is performed in less than
100 days, but the diminution between the same limits occupies
above 200 days.   It is under 13·5, and therefore invisible in our
equatoreal, for about 100 days ; but I have good reason to infer
from my projection that it does not go below the 14·3 mag., in
which case it would never be quite lost to a 10-inch object-glass.
At maximum it is of a dull red colour, but when near its vanishing
point is perfectly free from the hazy appearance presented at such
times by most other variable stars.   A well-watched maximum
occurred on 1854 Nov. 1, and another in Dec. 1855.   A small
star (10th mag.) 1″·5 np. 2ˢ, being equally affected by the bright-
ness of *θ Cygni*, is the best reference star for photometric com-

parisons with the variable, when they do not differ above 1 or 2 magnitudes in brightness."

" 49. *δ Cephei.* Argelander considers this the most regular of all known variables, and has given for its period 5ᵈ 8ʰ 47ᵐ 39ˢ·5. In 1851, Mr. Johnson made a series of photometric measures of *δ Cephei* with the heliometer, which are given in Vol. xii. of the *Radcliffe Observations.* He thence deduced for the epoch of maximum, 1851, Aug. 22·84, and the period, 5ᵈ 6ʰ 42ᵐ 18ˢ·4, which has been adopted in the table. Either period will reconcile distant observations equally well, but that deduced by Mr. Johnson was more in harmony with the results of his consecutive series than Argelander's. The interval between maximum and minimum is 2·95 days."

" 53. *R Cassiopeiæ.* This star was first observed by Mr. Johnson with the transit instrument on 1850, Nov. 29, and recorded as of the 6·5 mag. In 1852 it was several times looked for, but not seen, and accordingly supposed variable, but not proved to be so till September 1853, when I found it sufficiently bright (8·5) to bear observing with tolerable illumination. It rapidly diminished, falling through 3 entire magnitudes in about 40 days. In May 1854 it as rapidly increased, till about the end of June, when it was a fine flashing 7th mag., *vividly red,* forming a striking contrast to the white stars of similar magnitude in its neighbourhood. It occupied about 190 days in gradually diminishing to the 11th mag., when it began to waver, and show much unsteadiness till its disappearance, about the middle of Feb. 1855. After remaining invisible for 60 or 70 days, it reappeared as an extremely faint and hazy-looking object, which, when adjacent minute points of light were quite distinct, always seemed to be out of focus. At its last maximum, in September 1855, it was of a deep rich red or crimson. In comparing it photometrically by the method of reduced apertures with the two white stars 8307 and 8326 B.A.C. I learnt a curious fact. When *R Cassiopeiæ* was unmistakeably brighter than the former, and estimated exactly equal to the latter of these two stars, on gradually reducing the aperture of my object-glass, the variable was the first to vanish, however frequently the measure was repeated. Hence it appears, that of two stars, apparently equal in size or brightness, but one white and the other red, the white one is visible through a smaller aperture than the red one. It has since occupied nearly four months in diminishing to the 9th mag. *R Cassiopeiæ* is a double star; the companion being about 0'·8 np. 1ˢ, of the 11th mag. and not variable. On account of its intense colour, this is unquestionably the most remarkable variable yet found at this Observatory."

Mr. Pogson concludes his paper with a detailed account of the method of observation practised by him.

Note on the Telescopic Appearance of the Planet Mars. By the
Rev. T. W. Webb.

(Extract of a Letter to Mr. De La Rue.)

" I have had two or three pretty good views of *Mars,* and was
much interested by finding that at one
time (about April 15) there were, besides
the pole, three other brighter spaces close
to the limb, giving *something like* the
effect I have attempted to sketch. Curi-
ously enough, when last in London, I
found an old figure of *Cassini's,* in which a
similar appearance is represented (as far as
the bright spots are concerned), and one
could almost think that such may have
been the origin of the ancient observation (I forget at this
moment by whom) of *Mars,* as a ' quadrilateral rock vomiting
fire.' This, however, would, perhaps, be ascribing too much per-
fection to the earliest refractors.
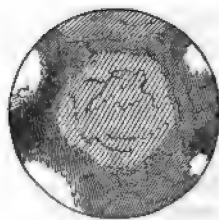
" *Tretire Rectory, May 8, 1856.*"

Elements of Harmonia. By M. C. F. Pape.

Epoch 1856, May 1·45198 M. T. Berlin.

$$
\begin{array}{lrrr}
 & \circ & \prime & \prime\prime \\
\text{M} & 193 & 8 & 43\cdot3 \\
\varpi & 10 & 45 & 38\cdot2 \\
\Omega & 93 & 8 & 17\cdot6 \\
i & 4 & 17 & 3\cdot2 \\
\varphi & 2 & 45 & 11\cdot0 \\
\end{array}
$$

M. Equinox,
January 0·0, 1856.

Log α 0·355603

Log μ 3·016603

Captain Shee continues to observe the Solar Spots every day on
which the sun is visible, and forwards his observations to the
Society. These consist merely of eye estimations of the relative
positions of the spots, and do not lay claim to any accuracy, but
perhaps they may be useful to some extent in confirming or dis-
proving conclusions derived from other sources.

Professor C. Piazzi Smyth has been authorised by Government to proceed to the Island of Teneriffe for the purpose of making Physico-Astronomical Observations on the summit of the Peak. The Council of the Society have been invited to furnish suggestions calculated to promote the objects of this expedition.

The Minor Planets.

Lieut. Maury, U.S.N., in a letter to Dr. Peters, which appears in No. 1026 of the *Astronomische Nachrichten*, proposes to astronomers a division of labour with respect to the observations of the Minor Planets. In pursuance of this plan he pledges the Washington Observatory for the requisite observations of eight of those bodies, selecting for this purpose *Egeria, Irene, Phocea, Fides, Psyche, Melpomene, Circe,* and *Thetis.*

He adds that in the meantime observations will be made at the same observatory of as many as practicable of the other members of the family, during the first and second oppositions succeeding discovery.

Dr. Peters responds to this proposal by announcing that henceforward the following eight planets will be regularly observed at the Altona Observatory:—*Hebe, Iris, Hygeia, Eunomia, Parthenope, Fortuna, Amphitrite, Lætitia.*

A gentleman wishes to dispose of a $5\frac{1}{2}$-foot refractor by the younger Tulley. It has an aperture of 3·7 inches, and is equatoreally mounted. It will elongate satisfactorily 36 *Andromedæ*, and show 31 *Canis Minoris* (Bode), in contact or just separated. For further particulars apply at the Apartments of the Society.

ERRATA.

Page 152, line 18, *for* objections *read* observations.

— — 30, *for* η^2 *Andromedæ, read* γ^2 *Andromedæ.*

CONTENTS.

ROYAL ASTRONOMICAL SOCIETY.

MANUEL J. JOHNSON, Esq., President, in the Chair.

Thomas Lee, Esq., Kilmarnock;
Rev. Bartholomew Price, Pembroke College, Oxford; and
George Rennie, Esq., 21 Whitehall Place;
were balloted for and duly elected Fellows of the Society.

New Planet.

A new planet was discovered on the 28th of May, by Mr. Norman Pogson, First Assistant at the Radcliffe Observatory, Oxford.

It was first seen on the 23d of May; but clouds prevented any further observation than a mere estimate of its place, which was then

		R.A.	N.P.D.
	h m	h m s	o '
May 23	12 30	16 13 6	105 23

It is rather brighter than a tenth magnitude. The President having been invited to assign a name to the planet, has proposed to call it *Isis.*

Observations of the planet made by Mr. Pogson on the evening of its discovery, and subsequently to that date, will be found at

Occultations observed by Lieut. Joseph Dayman, R.N.

(*Extract of a Letter to the President.*)

" The papers of the Royal Astronomical Society are sometimes sent to me by the Hydrographic Office at the Admiralty, and in that for April this year I see some occultations observed by Capt. Shadwell at Portsmouth.

" Having made a few observations of the same kind on the coast of South Africa, for the determination of longitudes, which have been calculated at the Royal Observatory at Greenwich. I

send them in the hope that they may be made useful through the same channel, if you should think them worthy of publication :—

Station of Observation.	Phenomenon Observed.	Day of Obs.	Observed Time of Station.	Resulting Long. East of Greenwich.	Latitude of Obs. Station.
		1854	h m s	h m s	o ′ ″
D'Urban, Port Natal }	Imm. of *ι* Ophiuchi	Aug. 4	9 16 18	2 4 16·9	29 52 48 S.
——	Em. of *ι* Ophiuchi	...	10 35 25	2 4 10·8	"
Port Elizabeth, Algoa Bay }	Imm. of *δ* Libræ	1855 April 4	13 16 11·4	1 42 18·6	33 57 36 S.
——	Imm. of *α²* Libræ	April 4	13 25 44·4	1 42 21·4	
——	Imm. of *φ* Sagittarii	April 8	14 34 28	1 42 21·9	

"The solar eclipse of November 20th, 1854, was also observed, but has not been calculated yet : the mean time of each contact was,—

Station of Observation.	Phenomenon Observed.	Day of Obs.	Observed Time of Station.	Latitude of Obs. Station.
		1854	h m s	o ′ ″
Port Elizabeth, Algoa Bay	Beginning	Nov. 19	22 24 57	33 57 36 S.
——	Ending	Nov. 20	0 57 16	

"All these observations were made with a refracting telescope by Fraunhofer.

"*2 Adelaide Street, Charing Cross, 1856, June 17.*"

Results derived from an Examination of certain places of the five principal Planets, as interpreted from Inscriptions on four old Tablets discovered in Egypt, made principally for Determination of the Epoch of the Tablets. By Mr. William Ellis, Assistant at the Royal Observatory, Greenwich.

(*Communicated by G. B. Airy, Esq., Astronomer Royal.*)

The tablets considered in this paper were discovered with other antiquities by the Rev. Henry Stobart during travels in Egypt in the years 1853 and 1854. The whole of these antiquities appear to have been afterwards submitted to the inspection of M. Brugsch, of Berlin, who seems to have soon discovered that the inscriptions on four small tablets, forming part of the collection, consisted of a long series of places of the five principal planets. The result of M. Brugsch's examination of these tablets is to be found in a pamphlet published at Berlin about the end of the year 1855, under the following title :—*Nouvelle Recherches sur la Division de l'Année des Anciens Egyptiens, suivie d'un Mémoire sur des Observations Planétaires, consignées dans quatre Tablettes Egyptiennes en écriture démotique.* M. Brugsch gives in this pamphlet a description of the appearance of the four tablets, accompanied by engraved copies of the same, and also his rendering of the Egyptian inscriptions, the epoch of them, however, being unknown,

The following extract from the pamphlet will give some idea of the tablets :—

"I discovered in this collection four tablets of wood. . . . . . These tablets, the edges of which are raised a little above the rest of the surface, exhibit on both sides a great number of demotic inscriptions, written upon a coating of plaster, some in columns of black ink, and others in columns of red ink, in a state of good preservation, with the exception of some parts where the plaster has been detached, or where it has been concealed by a greyish brown crust. One of the sides of each of these four tablets is pierced with two holes in three different places, a circumstance which leads one to suppose that they were originally bound together, so as to form a sort of book.

"Upon the first inspection which I made of these inscriptions in presence of M. Henry Stobart, I at once ascertained that they contain a very rich series of astronomical observations relative to five stars."

M. Brugsch was led to conclude from an examination of these inscriptions,—

1. That the eight pages of the four tablets relate to astronomical observations made in the years 8–19 of the reign of an Egyptian king, and of the year 1–17 of his successor. 2. That these astronomical observations relate to the five planets. 3. That the large number of demotic figures which follow the name of each planet represent two series, of which the first does not exceed twelve, and the second is not greater than thirty. The conclusion that the first series serves to indicate the twelve months of the Egyptian year ; and the second, one of the thirty days of each month, is justified by the circumstance that in some instances we find, in the second series, the sign *o* (expressing in demotics the word *day*) followed by one of the numerical signs serving to designate the days of the month. 4. That the five epagomene days are there mentioned for the first time in demotic writing, and are employed for the first time in fixing dates in general. 5. That the series of dates of the days of the month placed under the name of each planet would appear to indicate the day of the planet's entrance into one of the signs of the zodiac, or its retrogradation out of the sign.

It should be mentioned that after the figures representing the month and the day, there follows a character indicating the particular sign of the zodiac. M. Brugsch gives his rendering of the complete series of planetary places, which extend over a period of 29 years. The order in which the planets appear in each year is as follows :—*Saturn, Jupiter, Mars, Venus, Mercury.* There are, however, many gaps in the places during the later years, arising from causes already mentioned. After a discussion relating to the Egyptian names for the five principal planets, M. Brugsch says :—

"I have confined myself to establishing as rigorously as possible the Egyptian names by which the planets were designated,

It now remains to ascertain by calculation the epoch at which the observations consigned in these tablets were executed."

M. Brugsch concludes from the words "the year of the great house," which he met with in the course of his researches (an expression very frequently employed to designate the Roman emperors), that the tablets were written during the time of a Roman emperor. He remarks that the style of writing also applies very well to that epoch. The year 1 he accordingly concludes to be the first of the reign of an emperor. The year 19, which precedes, ought very naturally to designate the last year of the reign of his predecessor. This indicates the two reigns of Trajan and Hadrian, the former of whom reigned from 97 A.D. to 116 A.D., that is to say 19 years.

The following extract from the inscriptions on Tablet I., as interpreted by M. Brugsch, will serve as a specimen of the whole. The two sets of numbers in each column signify respectively the months and days according to the Egyptian calendar.

Name of Planet.	Years.						
	9	10	11	12	13	14	15
Saturn	1 1 ♐	1 1 ♑	1 1 ♑	1 1 ♒	1 1 ♒	1 1 ♒	1 ♓
—	3 5 ♑	...	5 14 ♒	...	7 24 ♓	3 24 ♓	4 ♈
Jupiter	1 1 ♌	1 1 ♍	1 22 ♎	1 1 ♎	1 1 ♏	1 1 ♐	1 ♑
—	12 28 ♍	...	...	2 21 ♏	3 18 ♐	4 5 ♑	18 ♒
—	...	...	...	...	...	8 15 ♒	5 ♓
—	...	...	...	...	...	10 26 ♑	
Mars	1 16 ♐	1 1 ♌	1 1 ♐	1 17 ♍	1 5 ♒	1 1 ♍	1 1 ♈
—	2 27 ♑	2 11 ♍	2 5 ♑	3 5 ♎	3 19 ♓	2 13 ♎	5 25 ♉
—	4 5 ♒	3 29 ♎	3 13 ♒	4 23 ♏	5 2 ♈	4 1 ♏	7 11 ♊
—	5 13 ♓	5 27 ♏	4 21 ♓	6 7 ♐	6 15 ♉	5 13	8 27 ♋
—	6 23 ♈	12 28 ♐	6 2 ♈	7 29 ♑	8 3 ♊	6 21 *	10 21 ♌
—	8 3 ♉	...	7 13 ♉	9 23 ♒	9 21 ♋	8 3	12 15 ♍
—	9 19 ♊	...	8 29 ♊	...	11 14 ♌	9 0	
—	11 1 ♋	...	10 13 ♋	...	Epag.3 ♍	11 6	
—	12 22 ♌	...	12 1 ♌				
* or 25.							

In the autumn of the year 1855, a manuscript copy of M. Brugsch's interpretation of the inscriptions on three of the eight sides of those Egyptian tablets was sent to the Astronomer Royal, who expressed a desire to have an examination made of the series of planetary places therein contained. The author accordingly *obtained* some results from them, which having come under the

notice of M. Biot, that distinguished philosopher kindly furnished him with a copy of M. Brugsch's pamphlet, in consequence of which he was enabled to examine the whole series of planetary places.

In order to discover, if possible, the probable positions of the planets in the different signs on the Egyptian days named; whether entering the signs, or whether in any other particular parts of the signs; the Egyptian places of *Mercury* and *Venus* for several instances of inferior conjunction where retrograde motion appeared to be shown, were laid down in curves, together with the curves described by those planets at some inferior conjunctions in recent times, for comparison; and it was found that the retrogradation at inferior conjunction could only be properly represented, on the supposition that the Egyptian times referred to the time of entry of the different planets into the various signs, at the commencement of the sign when moving direct, and at the termination of the sign when moving retrograde. On any other supposition the places could not be made properly to correspond. It was, therefore, assumed, that in all cases the Egyptian times referred to the time of entry of the planets into the respective signs; at the commencement or termination of the sign according to the motion of the planet at the time.

For determination of the epoch of the planetary places, the following was the course pursued:—An examination of the places of the quick-moving planets, *Mercury* and *Venus*, showed, that at the beginnings of the Egyptian years these planets were generally in or near to the signs ♌, ♍, or ♎; as if the year commenced near to the autumnal equinox: a more attentive examination showed that at the commencement of the year the Sun must have been near the beginning of the sign ♍. For finding the epoch by trial, it was therefore assumed that the first month of the Egyptian year corresponded to the month of September, Julian reckoning; which assumption proved to be very nearly true. Then, on the supposition that the vague Egyptian year, consisting of 365 days, was used, the places would refer either to some time near to 1500 B.C., or to about 30 B.C. But supposing the fixed year to have been used, the places might refer to any time later than the time of the alterations of the Egyptian calendar made about 25 or 30 B.C. Therefore, admitting both suppositions, the places are confined either to about 1500 B.C., or to some time later than about 30 B.C. M. Brugsch's conjecture that the end of the first series of planetary places corresponds to the end of the reign of the Emperor Trajan, points to the latter period as the more probable one; independently of which, the former is so remote as to render it on that account alone less probable. Moreover, the zodiac was most probably not known in Egypt at so early a period. The later time was, therefore, first examined. The epoch was definitely fixed by means of the places of *Jupiter* and *Saturn*, in connexion with the places of *Mars*. The Egyptian places seem to show that at some time,— certainly in year 17, in the first series,—there was a conjunction

in geocentric longitude of the planets *Jupiter* and *Saturn* in the sign ♈. But the geocentric longitudes of these planets never differ very greatly from the heliocentric longitudes. And as heliocentric conjunctions of *Jupiter* and *Saturn* occur at intervals of 20 years; each third conjunction, or those 60 years apart, taking place at an advance on the preceding one of 8° of longitude on the average, the intermediate conjunctions occurring at points 123° distant; it follows that in any particular sign of the zodiac, a series consisting of several conjunctions will take place at intervals of 60 years; after which no conjunctions will again happen in that particular sign for a space of about 700 years. On calculating, it was found that at about the commencement of the present era there were conjunctions of *Jupiter* and *Saturn* taking place in or near the sign ♈. Consequently if one of the conjunctions occurring near this time be not the one of year 17, it will be necessary to go backward or forward about 700 years, before conjunctions are found again to take place in ♈.

The approximate times of six successive heliocentric conjunctions near to the sign ♈ at the commencement of the present era, were in consequence calculated for trial, so as to include all geocentric conjunctions taking place in the sign ♈, with some which would occur in the signs ♓ and ♉. For each of these times the geocentric longitude of *Mars* was also calculated. Then assuming each conjunction in succession to have been that of year 17, the calculated places of *Mars* were compared with the corresponding Egyptian places in year 17, on the supposition that the Egyptian year commenced on September 1; comparing, however, those two instances so near the end of the Egyptian year with the places also from year 16. The results of the comparisons for the year 16 were found to be incongruous; those for the year 17 are contained in the following table :—

Approximate Longitude of Heliocentric Conjunction of Jupiter and Saturn.	Approximate Julian Time of Heliocentric Conjunction of Jupiter and Saturn.	Corresponding Egyptian Month and Day assuming 1st Month, 1st Day = Sept. 1.		Approx. Egyptian Longitude of Mars for preceding Egyptian Time from Year 17.	Cal. Geoc. Long. of Mars for Conjunc. of Jupiter and Saturn.	Difference between Egyptian Long. and Cal. Long for Year 17.
o				o	o	o
348	−6 Aug. 26	12th Month, 30th Day		173	217	44
357	54 Mar. 20	7th —	21st —	84	106	22
7	113 Oct. 22	2d —	22d —	69	67	2
18	173 May 9	1cth —	1st —	116	322	154
28	233 Jan. 6	5th —	8th —	56	20ʙ	145
39	292 Aug. 22	12th —	27th —	172	101	71

In consequence of the very near agreement of the longitudes in the third instance, the place of *Venus* was calculated for 113 October 22; and the place of *Mercury* for 113, November 13; and compared with the corresponding places in year 17, supposing first month, first day = 113, September 1. The calculated geocentric longitude of *Venus* was found to be 195°, and that of *Mer-*

cury 221°. The Egyptian places of *Venus* for 2d month 22d day, and of *Mercury* for 3d month 14th day, the times corresponding, were found to be 195° and 210° respectively; the difference for *Venus* being = 0°; and that for *Mercury* = 11°. Thus the places of all the planets agree within comparatively small limits on the supposition that year 17 of the tablets corresponds to 113 A.D., the difference for *Mercury* amounting only to a few days' motion. For each of the other conjunctions the place of *Mars* alone is discordant. It was consequently assumed that

<div style="text-align:center">

Egyptian.   Julian.

Series I.   Year 17, 1st Month = 113 September

</div>

and this makes the end of the first series of Egyptian years correspond to the end of the reign of the Emperor Trajan, as was conjectured by M. Brugsch. The preceding examination shows the certainty with which the identification would have been made in the absence of any conjecture as to the probable epoch; and also how the places could be proved to correspond to no other epoch.

Having found the epoch of the planet places, the approximate commencement of the year, and also that the Egyptian times referred to the time of the different planets entering the zodiacal signs, a more accurate determination of the Julian commencement of the Egyptian year was attempted as follows. The Julian times of entry of the planets *Mercury* and *Venus*, into various signs, were found by calculation to the nearest civil day, taking that day on which the entry occurred nearest to noon, for several instances in every year, excepting those contained on Tablet III. The times taken were when the planets were moving direct and with tolerable rapidity, no other particular selection being made. Then by counting back the number of days elapsed from the beginning of the Egyptian year to the Egyptian time corresponding, the Julian time of commencement of the Egyptian year was inferred, as in the following instance in year 10 of the first series, for *Venus*.

Egyptian Date and Sign.	Calculated Julian Time of Entry of ♒ corresponding.	Inferred Julian Time of Commencement of Year 10.
Year 10, 7^m 1^d ♒	107, February 26	106, August 30

The mean date of the commencement of the Egyptian year found in this way from a great many places of *Venus* was:—

<div style="text-align:center">

August 31$^{d.}$4

</div>

The places of *Mercury* treated in a similar way gave for the date of commencement of the year.

<div style="text-align:center">

August 28$^{d.}$2

</div>

The result found from the places of *Mercury* thus differs by several days from that found from the places of *Venus*. A consideration, however, of the results as given by each planet

separately, shows that the Egyptian year here used was evidently
of equal length with the Julian year. It was, probably, the fixed
Alexandrian calendar, as described in M. Biot's *Résumé de Chro-
nologie Astronomique*, Vol. XXII. of the *Mémoires de l'Académie
des Sciences*, in which the year commenced on August 29 Julian.
The mean of the places of *Venus* and *Mercury*, August 29^{d}·8,
very nearly agrees with this date.

The author next proceeds to ascertain the date of the com-
mencement of the year from the inferior conjunctions of *Mercury*
and *Venus*, and from the recorded entrance of the superior planets
into the various signs of the zodiac. The results are pretty
accordant, and serve to confirm the conclusion at which the author
had already arrived in reference to this point.

With respect to the question whether the Egyptian places are
the results of observation or of calculation, the author has the
following remarks :—

"As to the probability of the Egyptian places having been the
result of actual observation, an examination of the places them-
selves seems to show that they do not possess the character of
observations ; the places of *Mars* being continuously given at each
time of conjunction with the sun, when the planet for some time
could not have been visible : those of *Jupiter* and *Saturn* are also
occasionally given very near to the time of conjunction ; and those
of *Mercury* and *Venus* appear to form nearly unbroken series, ex-
cepting that the instances of retrogradation of *Mercury* at inferior
conjunction do not form an average, even when a deduction is
made for those cases in which the retrogradation would be confined
to the limits of one sign : in the case of *Venus*, however, there is
quite an average number of retrogradations noticed. The curious
discordances on Tablet III.* would also appear to show that the
places are not the result of observation. They seem, indeed, to
form a kind of planetary ephemeris, in which are given the suc-
cessive times of entrance of the fivĕ planets into the 12 signs of
the zodiac, as found by calculation, or by some equivalent method."

The author thus sums up the results which he considers to be
derivable from the preceding examination :—

"1. That the dates attached to the different signs of the zodiac
denote the times at which the planets entered those signs, accord-
ing to the Egyptian calendar ; either with direct motion at the
commencement of the sign, or with retrograde motion at the ter-
mination of the sign, as the case may be ; excepting those instances
in which the positions of the planets are given on the first day of
the Egyptian year, in which cases the proper signification is
merely, that at the beginning of the Egyptian year the planets
were within the limits of the sign named, and not at the entrance
of the sign : also, that as far as can be ascertained the position of
the Egyptian equinox was correct.

* The author gives a detailed account of these discordances in a preceding
section of his paper.

" 2. That the first day of the Egyptian year corresponded, throughout the series, either to about August 29, or August 30, of the Julian calendar, but with an uncertainty as respects the exact day ; the Egyptian calendar, however, being probably identical with the fixed Alexandrian calendar.

" 3. That year 9 of the first series commenced in August, 105, A.D., making the first series of Egyptian years correspond to the termination of the reign of the emperor Trajan, as was conjectured by M. Brugsch.   Consequently :—

		Egyptian.				Julian.	
Series I.	Year 9	1st month,	1st day	= A.D.	105	August 29 or 30
—	— 19	—	—	=	115	—	—
Series II.	Year 1	—	—	=	116	—	—
—	— 17	—	—	=	132	— .	—

" 4. That the Egyptian places are either calculated places, or places found by some equivalent method ; and are not the result of actual observation.

" With respect to the accuracy of the places calculated for comparison with the Egyptian places, it may be mentioned that in calculating the heliocentric longitudes, the equation of the centre was always applied: for *Jupiter* and *Saturn* the great equation was also applied: no others were taken into account.   In finding the geocentric longitudes, the true radius vector was always used, excepting for some of the places of *Venus* first calculated, for which the mean distances of the Earth and *Venus* were used."

Occultation of Antares.   Observed by Capt. Shadwell, R.N.

<div align="center">

Portsmouth M.T.

h   m   s

Disappearance (dark limb)   11   23   24·2

Reappearance (bright limb)   12   41   29·4

</div>

" At the disappearance the weather was quite clear, and the phenomenon was instantaneous.

" At the reappearance a slight haze was passing over the moon, but the time is probably true within a second.

" *R. N. College, Portsmouth, June 16th, 1856.*"

Occultation of Antares, June 16th, 1856.
Observed by T. W. Burr, Esq.

" Notwithstanding that the night was generally unfavourable for observation — the sky being mostly covered with hazy clouds

—a small space round the moon became clear prior to the occultation, which was therefore very well seen.

<div align="right">

Sidereal Time.
h m s

</div>

The immersion took place at the dark limb at   17 10 2
The reappearance at the light limb at 18 27 44

" The immersion was perfectly instantaneous.

" In consequence of the strong light of the moon, which was nearly full, and the emersion taking place at the bright limb, I had no chance of perceiving the small star with the telescope employed, my equatoreal of 4 feet focal length and 3⅝ inches aperture. The power used was 173.   Longitude 23ˢ·8 W.

" *Highbury, 8th July, 1856.*"

Observations of the New Planet Isis, made with the Ten-feet Equatoreal and Ring Micrometer, at the Radcliffe Observatory, Oxford. By Mr. Norman Pogson.

<div align="center">

(*Communicated by the President.*)

</div>

Greenwich M.T.	App. R.A.	App. N.P.D.	Logarithm of Par. Corr.×Δ.		Star of Comp.
			R.A.	N.P.D.	
h m s	h m s	° ′ ″			
May 28 12 33 44	16 7 36·07	...	+8·884	...	a
13 3 0	16 7 34·73	105 30 50·7	9·084	−0·893	a
13 37 19	16 7 33·14	105 30 54·7	9·238	·788	a
June 1 11 26 47	16 3 16·07	105 39 19·0	7·206	·898	b
11 26 47	16 3 16·39	105 39 19·1	7·2c6	·898	c
2 13 1 36	16 2 7·68	105 41 48·2	9·197	·890	c
3 13 44 25	16 1 1·93	105 44 14·8	9·349	·881	c
6 12 23 59	15 57 57·87	105 51 41·5	9·080	·895	c
9 13 29 57	15 54 54·96	106 0 10·7	9·384	·879	d
13 46 3	15 54 54·46	106 0 13·3	9·417	·874	d
10 12 8 7	15 54 0·38	106 2 59·0	+9·136	−0·893	d

All the observations have been duly corrected for refraction and motion.   The adopted places of the comparison stars, for the epoch 1856·0, were as follows : —

	Mag.	R.A.	N.P.D.
		h m s	° ′ ″
a = A.Z. 297.78 = 29610 Lal.	8	16 7 42·92	105 31 11·7
b = A.Z. 205.73	9	16 4 39·43	105,38 28·3
c = A.Z. 205.70 = A.Z. 297.70	9¼	16 0 31·22	105 42 19·8
d = A.Z. 205.66 = A.Z. 297.65 = 29175 Lal.	8½	15 55 22·60	105 59 55·4

Argelander's positions have been allowed double weight to those of Lalande.   The right ascension of star *d* is one minute too small in Argelander's Zone 297.*

* With the exception of *Flora*, *Isis* is the nearest to the sun, and has, therefore, the shortest periods of the known asteroids.

On June 3 *Isis* passed exactly over a star of the 10th magnitude, at 13^h 16^m Mean Time. The night being hazy and definition consequently very bad at so low an altitude, a power of 65 was preferred, though much too small for such an observation.

At 13^h 12^m 20^s G.M.T. *Isis* was so close to the star as to be inseparable, though decidedly elongated.

At 13^h 16^m 20^s, the two objects appeared as one sharp round star.

At 13^h 19^m 20^s, they again began to look elongated.

At 13^h 21^m 20^s, the planet and star distinctly separated.

The mean of the first three times, viz. 13^h 16^m 0^s, may be taken as the true time of occultation. If the planet had passed either above or below the star, instead of occulting it, a rapid twisting of the angle of position would have been evident. Nothing of the kind was, however, seen ; and it may be safely assumed that a good meridian position of this star will fix the place of the planet at the above time with great accuracy. Its approximate mean place for 1856·0 is,—

R.A. 16^h 1^m 1^s N.P.D. $105°$ $44'$

On the Variability of 13 *Lyræ*. By Josh. Baxendell, Esq.
(*Communicated by Sir John Herschel.*)

" In December last I was led to suspect that 13 *Lyræ* was subject to a slight periodical change of brightness. A series of observations, which I have since made, has confirmed the suspicion, and given an approximate period of 48 days. The range of variation is about 3-10ths of a magnitude, the highest maximum which I have yet observed being 4·28, and the lowest minimum 4·60. The last minimum occurred on the 14th of June. Like many of the other variables, 13 *Lyræ* belongs to the list of ruddy stars.

" *Manchester, July 3d, 1856.*"

On the Period and Changes of α *Herculis.*
By Josh. Baxendell, Esq.
(*Communicated by Sir John Herschel.*)

The period in which α *Herculis* completes all its changes of brightness was supposed by Sir Wm. Herschel to be about $60\frac{1}{4}$ days. More recently M. Argelander has been led to conclude that it may be estimated at 66 days 8 hours. Some years ago I found that Sir Wm. Herschel's period would not satisfactorily represent a number of observations which I had occasionally made ; but assuming it to be sufficiently near the truth to serve as an approximation, I obtained a mean period of 63 days. Although this period was decidedly preferable to one of $60\frac{1}{4}$ days, or indeed to any

other between the limits of 50 and 70 days, yet some anomalies still remained, which, from the desultory nature of the observations, I was unable to account for in a satisfactory manner. In order, therefore, to ascertain the cause of these anomalies, I have since made a more regular and more extensive series of observations; and have arrived at the very remarkable and unexpected result—that the periods assigned by Sir Wm. Herschel and M. Argelander are both considerably in error; and that, in fact, the actual mean period is about 88½ days. The observations which have led to this result extend over a period of nearly *eight* years, and have been made with every precaution to avoid the errors to which such observations are liable. The stars which have been used in the comparisons are :—

β Herculis	3·14	Magnitude.	
ζ ——	3·19	—	
β Ophiuchi	3·26	—	
δ Herculis	3·51	—	
κ Ophiuchi	3·58	—	
μ Herculis	3·69	—	
ξ Herculis	3·91	—	

The comparisons have generally been made with at least two stars, and frequently with three or four; and as all the stars of comparison are not equally steady in brightness, they have been frequently compared with each other and with other stars in the neighbourhood, and whenever any change has been observed, it has been taken into account in making the reductions.

The number of single periods which the observations have afforded is *twenty-four*—namely, *eleven* from maximum to maximum; and *thirteen* from minimum to minimum. The longest single period is 111 days, and the shortest 70 days; the mean of the whole being 86·79 days. This value has been used in determining the number of periods in the longer intervals; regard being had, however, at the same time, to the indications presented by such observations as may have been made during the intervals.

The following tables include all the maxima and minima, the middle times of which have been obtained with sufficient accuracy to be available for determining the value of the mean period :—

Observed Maxima of α Herculis.

No.	Middle Times of Maximum Brightness.	Magnitude.	Intervals in Days.	Number of Periods in the Intervals.
	1848.			
1	Nov. 13	3·26		
	1849.			
2	March 4	3·28	111	1
3	June 13	3·30	101	1
4	Sept. 8	3·26	87	1
	1850.			
5	June 14	3·14	279	3
6	Aug. 26	3·29	73	1

No.	Middle Times of Maximum Brightness.	Magnitude.	Intervals in Days.	Number of Periods in the Intervals.
	1851.			
7	March 6	3·56	192	2
8	June 14	3·42	100	1
9	Sept. 12	3·45	90	1
	1852.			
10	Aug. 15	3·47	338	4
	1853.			
11	July 30	3·30	349	4
12	Oct. 25	3·32	87	1
	1854.			
13	April 20	3·14	177	2
14	July 21	3·30	92	1
15	Oct. 8	3·23	79	1
	1855.			
16	April 19	3·39	193	2
17	June 29	3·32	71	1
18	Dec. 25	3·30	179	2
	1856.			
19	Mar. 15	3·41	81	1

Observed Minima of α Herculis.

No.	Middle Times of Minimum Brightness.	Magnitude.	Intervals in Days.	Number of Periods in the Intervals.
	1848.			
1	Sept. 22	3·77		
	1849.			
2	April 26	3·69	216	2
3	July 29	3·52	94	1
4	Oct. 21	3·62	84	1
	1850.			
5	April 29	3·46	190	2
6	July 14	3·42	76	1
7	Oct. 10	3·58	88	1
	1851.			
8	April 20	3·87	192	2
9	Aug. 2	3·69	104	1
	1852.			
10	April 21	3·69	263	3
11	June 30	3·66	70	1
12	Sept. 30	3·63	92	1
	1853.			
13	June 16	3·63	259	3
14	Sept. 15	3·58	91	1
	1854.			
15	June 5	3·58	263	3
16	Aug. 19	3·46	75	1
	1855.			
17	May 17	3·54	271	3
18	Aug. 14	3·60	89	1
19	Nov. 13	3·48	91	1
	1856.			
20	Feb. 8	3·56	87	1
21	April 18	3·56	70	1

The data in these tables give the following results, the values of the mean period having been obtained by the method of least squares:—

Mean period by table of maxima ... ... = 88·63 days.
— — minima ... ... = 88·47

Mean = 88·55

Interval from minimum to maximum ... ... = 45·24
— maximum to minimum ... ... = 43·31

Sum = mean period = 88·55

Mean minimum magnitude ... ... = 3·59
— maximum — ... ... = 3·32

Mean range = 0·27

Least minimum magnitude ... ... = 3·87
Greatest maximum — ... ... = 3·14

Greatest range = 0·73

Mean range of variation in the length of the period = 11·87
Greatest range — — — = 41·00

During the interval from September 1848 to August 1852, the variations in the length of the period were very remarkable, the extreme range amounting to 41 days, and the mean to 14·9 days; while in the interval from August 1852 to April 1856 the extreme range of variation was only 26·5 days, and the mean 7·8 days. The average length of a period was also greater in the former interval than in the latter, the difference being about 4½ days.

Note on the Telescopic Appearance of Mars.
By Frederick Brodie, Esq.

(*Extract of a Letter to Mr. De La Rue.*)

"I enclose you a rough sketch of *Mars*, as seen by me on April 18th. I find it rather difficult to give just the representation that I would wish, without giving considerable time to it, my hand has been so long out of water-colouring that I can only profess to give outline correctly. It seems that the date corresponds to your own observations nearly. The colour should be properly made of *brickdust*. I send you an extract from the observatory book concerning it.

"*The Gore, 16th June, 1856.*

"April 18th. Wind east. Atmosphere unsteady. 10ʰ 10ᵐ Sidereal Time. Observed *Mars*, which is very near the moon

(13·3 days old). I never saw the planet to so much advantage. The Poles are brilliantly illuminated, especially the South Pole.
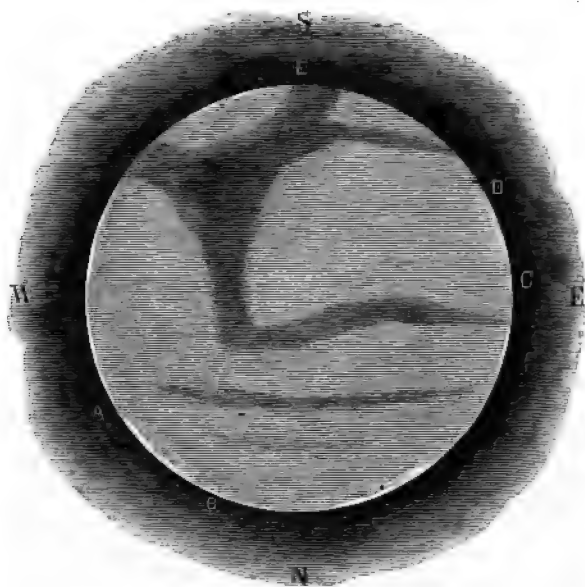
The dark part is of a dirty sea-green shade; the rest of the planet reddish, as usual. I saw the planet well with powers 396 and 578; object-glass 6¼ inches aperture. On further examination I see that the greater part of the periphery of the planet is illuminated, though not so bright as the illumination at the Poles, more especially from A to B and C to D, but all round from E to A B C D, ending at D, is very much brighter and whiter in colour than the rest of the planet. It now became cloudy."

The Fellows of the Society will be glad to learn that Lady Bellew has presented to the Society, through Sir John Macneill, a copy of the celebrated Tables of her father, Mendoza Rios, the eminent Nautical Astronomer, containing numerous autograph corrections by the author. Mendoza Rios, as is well known, was originally an officer in the Spanish service, but his attachment to England induced him to quit Spain and adopt this country as his future place of abode. It has been asserted that this excellent astronomer had a pension from the English Government, but there is reason to believe that this statement is not correct.

Allusion was made in the last *Monthly Notices* to an expedition which Government contemplated despatching to the Island of Teneriffe.   Professor Smyth, to whom the charge of the expedition was confided, has since proceeded to his destination.   The readers of the *Monthly Notices* will be gratified to learn that he was furnished with the means of transport thither by Robert Stephenson, Esq. the eminent engineer, who munificently placed his private yacht Titania at the disposal of Professor Smyth.

The Royal Academy of Sciences of Göttingen has presented to the Society two impressions of a medal struck in honour of the illustrious Gauss, one in silver and the other in bronze.   This interesting memorial is 2¼ inches in diameter.   On the obverse is seen the head of Gauss, with the inscription :

<div align="center">

CAROLVS FRIDERICVS GAVSS

NAT. MDCCLXXVII. APR. XXX. OB. MDCCCLV. FEB. XXIII.

</div>

Upon the reverse is a wreath of ivy, within which are to be read the words :

<div align="center">

GEORGIVS V. REX HANNOVERAE,

MATHEMATICORVM PRINCIPI.

</div>

Around the wreath we read :

<div align="center">

ACADEMIAE SVAE GEORGIAE AVGVSTAE DECORI AETERNO.

</div>

Suggestions respecting a Method for determining the Brightness of the Minor Planets.   By Professor Argelander.*

The author commences this Paper with some preliminary remarks on the importance of observations of the brightness of the minor planets.   It is plain that researches of this nature, if planned with intelligence and prosecuted with zeal for a considerable length of time, might lead to various important conclusions, not only with respect to those bodies themselves, but also relative to many other interesting questions of astronomy, —conclusions which are the more important the less reason we have to hope to arrive at them by any other means.   The author, accordingly, proceeds to give a few hints relative to the most suitable mode of conducting observations of this kind.   They are merely hints; but as they are founded upon long personal experience, they may not be without some use.   Moreover, an expert observer will have no difficulty in applying to them various

* *Ast. Nach.* No. 996.

modifications, which may adapt them more effectually to the
object he has especially in view.

The points which the author considers to be mainly worthy
of attention in observations of this kind are the following :

1. Since we have reason to suppose that all the small planets
revolving between *Mars* and *Jupiter* have had a common origin,
and have passed through the same stages of physical organization,
we may conclude that they are all characterised by the same, or
at least *nearly* the same, natural colour of surface. Hence the
determination of their relative brightness would serve to indicate
their relative magnitudes; and consequently, we might arrive at a
knowledge of their absolute magnitudes, if we should once be in a
position to determine the apparent diameter of one of them. It is
not improbable that, in the course of time, we may be enabled to
accomplish this object. No reliance can be placed upon any results
of this nature which have been hitherto obtained by observers;
or, at all events, they can only be considered as affording, in each
instance, a superior limit of the true measure. It cannot be
doubted, however, that the immense telescopes, which are now
directed towards the heavens in different parts of the world, will
enable the observer, under favourable circumstances, to arrive at
trustworthy results in relation to this object. Hence the deter-
mination of the *mean brightness*, and the comparison of the values
of that element for the different planets, must form one of the
principal objects of research.

2. It is well known that observers have suspected variations
in the brightness of several of the minor planets, which seemed to
indicate a rotation on an axis. The author himself had an-
nounced an observation of this kind, which appeared in No. 325
of the *Astronomische Nachrichten.* Such a conclusion, however,
is very doubtful, the influence of atmospheric circumstances being
very deceptive. The atmosphere, especially in our latitudes, is
scarcely ever free from vapours. Even during apparently the
most serene sky the heavens are here and there disturbed by very
minute vapours, the presence of which is recognised by the fact
that very faint stars suddenly vanish altogether, and soon re-
appear; while others in their vicinity, which were easily per-
ceived, now cease to be visible. It is manifest that such dis-
turbing causes must exercise an influence upon the brightness of
stars which do not vanish altogether. In observations with
illuminated fields of view, we have to encounter another difficulty
arising from the circumstance of the illumination being not al-
ways the same. This must be adapted to the brightness of the
object to be observed. The degree of faintness of the field
affords now, indeed, an estimate of the brightness of the star, but
a very imperfect one, since the eye very soon accustoms itself to
a faint illumination, and imagines it to be more intense than
it really is. It is only by direct and repeated comparisons of two
stars that we can hope to arrive at anything approaching to trust-
worthy results. In this way, however, we may succeed in de-

tecting real variations of light in the small planets. Should these arise from the existence of irregularities on the surfaces of the planets combined with a movement of rotation, they ought, as a necessary consequence, to return at regular intervals of time. It is possible, however, that they may be due to other causes. It seems, indeed, very probable, that the space between the planets of our system is not absolutely empty, but is pervaded more or less by a fluid substance, or by small bodies. If the orbit of *Mars* was encompassed by a system of aerolites similar to those which we perceive in the neighbourhood of the terrestrial orbit, or if such a system of aerolites revolved in the interval between *Mars* and the small planets, the light of the latter might undergo a sensible diminution, and we should thereby be enabled to detect the existence of the revolving bodies. Variations of light of this kind, as well as possible fluctuations in the atmospheres of the small planets, would be distinguished by an absence of periodicity from those which would be disclosed by a movement of rotation. In order to detect such variations, it would be necessary to institute comparisons at different times, partly in close succession, partly at wider intervals of time apart.

3. Our knowledge of the relative brightness of the stars in the different classes of magnitudes is still very imperfect. We know only very superficially how the different classes of magnitudes, according to which we are accustomed to arrange the stars, are related to the quantities of light which they transmit to the eye. The photometric determinations hitherto executed in reference to this object deviate so considerably from each other that they can only be regarded as rough approximations. This arises partly from the imperfect means which we possess for measuring light, especially when the question relates to faint stars, and partly from the difficulty of employing those means in the execution of large masses of observations, whereby the fluctuations in the condition of the atmosphere might be eliminated. To this we must add the different impression which differently coloured light produces upon the eye, the influence exercised by circumstances depending on the peculiar constitution of the observer's eye, and by the particular kind of telescope employed in the observations. Red light, for example, makes a considerably fainter impression upon the eye of a short-sighted than upon that of a long-sighted person; also in small telescopes the impression produced is fainter than in more powerful instruments. Now, the proposed method of observation must solve all these ambiguities. If we compare the small planets in every possible variety of distance from the sun and earth with a series of well-chosen fixed stars, and if we hence calculate according to photometric principles the quantities of light which we receive from the planets at the individual distances, we shall thereby become acquainted with the relations between the different magnitudes, at least up to the sixth magnitude (through *Vesta*), and thus a scale might be constructed which would serve as a guide to observers in the estimation of magnitudes. From such

materials there might be deduced much more trustworthy conclusions than have been hitherto arrived at respecting the general distribution of the stars according to their different distances and the arrangement of the stars in the system constituting the milky way. By means of observations of this kind the question might also be decided whether the celestial regions beyond our solar system are pervaded by a substance of unequal density, since the occasional interposition of the more compressed parts of such a medium, arising from the movement of the solar system in space, would cause the stars to assume a fainter appearance. Olbers, and in more recent times Sir John Herschel, have shown that such an hypothesis is by no means improbable, and would afford an easy explanation of many facts which have been established with a greater or less degree of certainty.

4. Imperfect as is our knowledge of the absolute brightness of the stars, the information which we possess respecting their relative brightness at different altitudes above the horizon is equally unsatisfactory. The law according to which our atmosphere absorbs the light of the stars at different altitudes has repeatedly formed the object of research since the times of Bouguer and Lambert, but it appears that notwithstanding even the careful researches of Seidel the requisite degree of certainty has not been attained. This arises chiefly from the fluctuations of our atmosphere, the injurious effects of which can only be eliminated by means of a large mass of observations. This circumstance must therefore be taken into account in devising a proper method of observation.

In order, then, to attain these various ends in greatest perfection, and as free as possible from constant errors, the author suggests the following course of procedure :—

1. First, it will be necessary to select a series of fixed stars of different magnitudes, which may serve as points of reference for comparisons of brightness. Such stars can only be found in the region of the pole, where, in consequence of their almost uniform, and in our latitudes pretty considerable altitudes above the horizon, the brightness of the stars is not sensibly altered by the extinction of the rays of light in the course of their passage through the atmosphere. The region between *δ Ursæ Minoris* and 24 *Cephei* (Hev.) is well adapted for this purpose. The latter star, and consequently the entire region, is easily recognisable by means of four stars situate near it, and forming the figure of a rhomboid.* The region comprises stars of all degrees of brightness from the sixth to the tenth magnitude. Even fainter stars

* The positions of these stars for 1855 and their approximate magnitudes in integers and decimals are the following :—

M	h m	° ′	M	h m	° ′
8·3	20 17	+ 88 33	9·3	20 30	+ 88 47
9·0	20 24	+ 88 26	8·5	20 55	+ 88 41

will be found in abundance if the observer should desire to include such in his comparisons.

2. A number of these stars, including every shade of brightness, must now be carefully compared together, and their relative brightness determined by means of repeated observations. The author alludes to the method which has been employed for effecting this object by comparisons of equal brightness. He remarks that, according to his own experience, one may judge with equal precision respecting minute gradations of light, and that results equally worthy of confidence will be obtained if the observer should compare the star whose brightness is to be determined with two other stars, one of which is a little brighter, and the other a little fainter. He accordingly proposes the method which he employs in the observation of variable stars, and which has been found by several other observers to be a convenient one. Designating the faintest of the stars by o, the next brighter by 1, 2, 3, &c., a scale of comparison will thereby be formed which will serve to ascertain the relative brightness of the small planets. By observing the planets at various distances from the sun and earth, materials will be obtained for comparing the luminous intensities of the stars of the scale with their estimated magnitudes.

3. It would not be advisable to compare the planet directly with the stars of the scale which approach it in brightness. Our knowledge of the light-absorbing power of the atmosphere at different altitudes is not only too uncertain, but the absorption varies too much with the time and the azimuth of the object to admit of this process. The observer, therefore, on every occasion, when he wishes to determine the brightness of a planet, selects two stars in its vicinity, one being a little brighter, and the other a little fainter than it. It is not necessary that these stars should be visible in the field of the telescope at the same time with the planet. It is even desirable to select them at some distance from the planet, preceding or following the latter, according as its motion is retrograde or direct, so as to be enabled to employ the same stars of comparison for several days. By this means the observer is enabled to recognise with greater facility the existence of slight periodic variations in the brightness of the planet, but the principal advantage which he derives from the process consists in the elimination of the effects of any irregularities in the condition of the atmosphere in the region of that planet and that of the scale. The stars which have been directly compared with the planet must now be compared with as many stars of the scale as possible, and it will be desirable to institute this latter comparison on the same nights that the comparisons of the planet have been made. It is only necessary to select such stars as form easily recognisable configurations, so that they may be found again without any difficulty. For this purpose it will be desirable that the observer should trace out upon a chart the course of the planet to be observed, and that he should make himself well acquainted

beforehand with the stars near which it passes. If, after the lapse of some time, the planet has removed too far from the first stars of comparison, or if its brightness has varied too considerably, the observer should then select new stars according to the same principles, comparing these not only with the stars of the scale, but also, if practicable, with the former stars of comparison, always endeavouring by a multiplication of comparisons to render the results more trustworthy. Should the planet after quitting a stationary point revisit the same region which it had previously .traversed, the observer must then search for stars approaching it in brightness, and compare these, if necessary, by means of other stars of intermediate brightness, with the former stars of comparison. Moreover, should several small planets happen to be all at once in the same region of the heavens—a circumstance which frequently occurs,—and should they not differ too much in brightness, the observer ought not to omit carefully comparing them together as often as occasion offers.

4. In applying this method of observation the process of comparing with the scale will be found to be very difficult, indeed the certainty of the result will be very much endangered, if the observer should have at his disposal only one telescope, which he turns alternately upon the scale and the region of the planet. Even if he should have an assistant to attend to the adjusting of the instrument, and if the latter should be furnished with circles for this purpose, still it will be impossible to prevent extraneous light from entering the eye of the observer. In every case the time which elapses between the two corresponding observations will be too long for the impression which the brightness of the first star has left in the eye to remain undiminished until the observer is enabled to consider the second star. In order to obtain a result which shall be in any degree worthy of confidence, a great many comparisons must be instituted, and each observation must occupy a considerable interval of time. The observer must abandon the use of very large telescopes in researches of this kind, and must content himself with instrument of moderate power. Telescopes from the Munich workshops of 48, and even 42 lines aperture, will amply suffice for accurate comparisons of stars to the tenth magnitude. They possess at the same time the advantage of being applicable to observations of the brightest of the small planets in all stages of brightness, which is not the case with respect to large refractors. In these the stars of the sixth and seventh magnitudes, and even the brighter stars of the eighth magnitude, have so much light that it is impossible to estimate their brightness with any degree of certainty.

The observer must now direct one of the telescopes to the scale, and the other to the region of the planet. The two instruments must be placed so that the eye-pieces shall be near to each other, and the eye in a few seconds can pass from the one to the other. It is a matter of importance that both telescopes should have the

same optical power; but as this object cannot be rigorously ac-
complished, it will be desirable that the observer should direct each
telescope alternately to the scale. When once the relative optical
powers of the two telescopes have been thus ascertained, the
observations may be henceforward prosecuted without having re-
course to an interchange of instruments.

5. It is important that the observations should be prosecuted
in the dark, and that no extraneous light should enter the eye
until the observation is quite finished. The observer must, there-
fore, arrange beforehand, so as to be enabled to write in the dark
without confusion. A framework or system of strips of black
paste-board, laid upon paper, will here perform good service, since
the strips of paste-board, which may be seen even in the darkest
nights, will serve as lines for separating the individual observations
from each other.

6. No observations should be made during the prevalence of
a fog, even although it should appear to be uniformly distributed
over the whole heavens. Nor should any be made during bright
moonlight, especially if the moon should happen to be anywise
near the planet which is being observed, since in that case the
illumination of the ground of the heavens would be too unequal
in the region of the planet and that of the scale. Still less should
observations be made at altitudes lower than $10°$. Near the
horizon the vapours arising from the surface of the earth, as well
as the radiation of heat from the latter, exercise too great or too
fluctuating an influence, and render the extinction of light too
uncertain, both absolutely and in different azimuths, to hold out
any prospect of deducing trustworthy results from such observa-
tions, even although repeated many times. Even the observations
comprised between $10°$ and $20°$ of altitude will for the most part
serve merely for ascertaining the extinction of light, and only in
exceptional cases and under especially favourable circumstances
can they be employed for the ultimate object of the investigation.

7. It will be desirable to observe only such planets as, at the
time of observation, have attained an altitude equal to that of the
scale, in order not to be too dependent on the law of the extinction
of light which has been found. In doubtful nights, observations
should be made only of such planets as have attained or exceeded
that height. In very bright nights, however, and especially when
the weather holds out the prospect of a continuance of clear nights,
observations should be made repeatedly, and at as different alti-
tudes as possible. The observations of the same star should not
succeed each other too quickly. After the lapse of an hour, or an
hour and a half, the observer will have already almost entirely
forgotten the numbers of the earlier observation, and he may,
therefore, institute a second observation, without having his mind
occupied with the one preceding.

These are the methods and precautionary measures which the
author suggests for the purpose of giving due efficacy to such ob-

servations. Their adaptation to the object of research will be easily seen; but the attentive observer will undoubtedly modify them to a considerable extent. They will attain the end which the author had in view, if they spare the observer a series of fruitless attempts.

The Minor Planets.

In conformity with the plan adopted at the Observatories of Washington and Altona, Professor Challis announces his intention of confining in future his observations of the minor planets with the Northumberland equatoreal to the following eight bodies: *Flora, Metis, Victoria, Themis, Proserpina, Bellona, Urania, Leucothea.*

Professor Challis purposes to observe the others occasionally, but to give these his particular and continued attention.

Dr. Gould, editor of the *Astronomical Journal* (U.S.), states (No. 94, p. 176,) that Professor Brünnow has undertaken, in behalf of Ann Arbor Observatory, of which he is director, the regular observation of the following eight asteroids:—

> *Flora, Victoria, Astrea, Metis, Proserpina, Calliope, Euphrosyne, Hebe.*

In order to facilitate and hasten this division of labour, Dr. Gould guarantees that four others shall also be regularly observed in the United States after the beginning of the year 1857; and he selects the following four as least likely to interfere with the preference of others:—

> *Vesta, Iris, Eunomia, Amphitrite.*

Dr. Gould remarks that these will complete for the western continent its contingent of one half of the known members of the group. He could not, of course, have then been aware of the recent discovery of two new planets, nor of the proposal of Professor Challis.

ERRATA.

Page 173, *Lætitia*, April 19, App. N.P.D., *for* 78° 49′ 47″·13, *read* 77° 49′ 45″·31.

— 185, line 13 from top, *for* zinc, *read* time.

CONTENTS.

Admiral R. H. MANNERS, Secretary, in the Chair.

On the Conjunctions of the Planets Jupiter and Saturn in the
   Years B.C. 7, B.C. 66, *and* A.D. 54. By the Rev. C. Pritch-
   ard, M.A., F.R.S.

In this paper the author has corrected an astronomical error,
into which Dr. Ideler and several others have fallen, in attempting
to establish the date of the true *Annus Domini.* The German
chronologist, in his *Handbuch der mathematischen und technis-
chen Chronologie,* has remarked, that certain conjunctions of the
planets *Jupiter* and *Saturn* wholly fulfil the conditions and
phenomena recorded of the star of the Magi. It asserts that of
three conjunctions which occurred in the year B.C. 7, the first
was of a nature sufficient to arouse the attention of the Magi, and
send them on their errand to Jerusalem, and that the last of the
three conjunctions was so close that, to weak eyes, the discs of
the two planets might appear diffused into one, and would satisfy,
moreover, the condition of being in a proper position at sunset to
conduct the Magi from Jerusalem to Bethlehem.

In order to ascertain the accuracy of this statement, the author
undertook the computation of the geocentric places of the two
planets for the year B.C. 7, so far as any possible conjunctions
were concurrent. The result is, that as regards the fact of there
having been three conjunctions during the year, Dr. Ideler's state-
ment is confirmed; but the author finds that the dates assigned
by Ideler to these conjunctions are not correct; still less is it true
that any such proximity occurred as to make it possible that the
planets could, to any observer, have presented the appearance of a
single star.

The following are the places of the planets for each of the
three conjunctions, as computed by the author.

December 4, B.C. 7, at 6 p.m., Paris mean solar time.
Sun's longitude, 250° 57′ 57″·9.

	Geoc. Long. True Equinox.	Geocentric Latitude.
	° ′ ″	° ′ ″
Jupiter	345 30 5·1 ..	1 28 27·9 S.
Saturn	345 33 44 ..	2 31 8·0 S.

September 29, B.C. 7, at Paris mean midnight.
Sun's longitude, 184° 17′ 7″·6.

	Geoc. Long. ° ′ ″	Geoc. Lat. ° ′ ″
Jupiter	347 35 5 ..	1 46 13 S.
Saturn	347 32 20 ..	2 44 18 S.

May 29, B.C. 7, at Paris mean midnight.
Sun's longitude, 64° 59′ 15″·4.

	Geoc. Long. ° ′ ″	Geoc. Lat. ° ′ ″
Jupiter	351 1 17·3 ..	1 20 57·3
Saturn	350 59 42·7 ..	2 19 57·1

From these results it would appear that in a latitude not differing much from that of Jerusalem, on Dec. 4, B.C. 7, the planets would be about 1½ hour east of the meridian at sunset, and would, on May 29, rise about 3½ hours before sunrise.

The author has computed other two ancient conjunctions, one of which occurred in the year B.C. 66, and the other in the year A.D. 54. The distance between the two planets on the occasion of the conjunction in the year 66 was found to be only 55′. With respect to the conjunction of 54 A.D., the planets were too near the sun to be visible.

The results of the author's researches were confirmed by calculations executed independently at the Royal Observatory by the instructions of the Astronomer Royal.

Results of the Observations of Small Planets made at the Royal Observatory, Greenwich, chiefly in July, August, and September, 1856.

Flora.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1856, June 28 11 0 21·38	17 29 13·63	109 32 6·96
July 1 10 45 25·30	17 26 4·78	109 38 46·12
2 10 40 28·60	17 25 3·82	109 41 1·33
3 10 35 33·18	17 24 4·14	109 43 18·44
16 9 33 45·70	17 13 21·74	110 14 56·72

Thalia.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	° ′ ″
1856, Sept. 16 14 20 30·60	2 5 20·67	90 3 31·42

Melpomene.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	° ′ ″
1856, Sept. 16 15 0 37·50	2 45 33·68	88 35 6·09
23 14 34 54·60	2 47 22·50	89 51 24·38

Euterpe.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	° ′ ″
1856, July 22 13 44 59·40	21 48 56·09	105 0 3·18
30 13 7 10·37	21 42 33·31	105 38 50·72
31 13 2 21·87	21 41 40·58	105 43 57·47
Aug. 5 12 38 7·20	21 37 4·70	106 10 13·86
7 12 28 20·52	21 35 9·54	106 20 46·83
12 12 3 47·25	21 30 15·02	106 47 17·89
14 11 53 56·71	21 28 15·97	106 57 38·67
Sept. 4 10 12 20·90	21 9 11·12	108 27 57·99
5 10 7 41·31	21 8 27·33	108 31 34·95

Metis.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	° ′ ″
1856, June 28 12 53 28·99	19 22 39·82	117 26 42·21
July 9 11 58 36·22	19 11 0·18	118 8 22·58
Aug. 5 9 47 2·43	18 45 31·83	119 3 16·64

Eunomia.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	° ′ ″
1856, Sept. 2 14 23 2·53	1 12 40·83	62 49 25·32
3 14 18 53·79	1 12 27·97	62 41 50·73
4 14 14 40·83	1 12 10·87	62 34 31·62
16 13 22 16·40	1 6 56·49	61 27 11·72
20 13 3 54·22	1 4 17·49	61 14 14·88
23 12 49 53·20	1 2 3·83	61 7 59·15
30 12 16 31·50	0 56 12·54	

Isis.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, June 7 10 50 58·06	15 57 1·03	105 54 12·75
11 10 31 21·44	15 53 7·42	106 5 45·56
14 10 16 51·76	15 53 25·04	106 15 21·59

Fortuna.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, Sept. 16 12 56 48·60	0 41 24·51	84 3 38·05
30 11 50 48·40	0 30 25·19	85 26 38·88

Astrea.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, July 3 13 32 25·85	20 21 25·87	106 6 37·66
29 11 27 32·32	19 58 42·33	107 49 16·76
Aug. 2 11 8 20·53	19 55 13·62	108 5 26·70
4 10 58 48·39	19 53 33·02	108 13 17·16
5 10 54 3·16	19 52 43·57	108 17 17·55

Irene.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, Aug. 2 13 55 5·99	22 42 26·47	110 47 9·07
4 13 45 53·02	22 41 5·11	111 1 24·29
5 13 41 14·11	22 40 21·98	111 8 37·77
7 13 31 55·23	22 38 54·70	111 22 48·37
14 12 58 40·41	22 33 20·34	112 11 41·48
Sept. 22 9 54 6·0	22 1 51·21	114 51 2·31

Calliope.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, Aug. 4 13 57 4·51	22 52 18·43	119 7 46·82

Circe.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, Aug. 5 11 18 58·54	20 17 43·04	102 29 51·49
21 10 4 42·82	20 6 20·02	103 36 40·33
28 9 33 (38·2)	··········	104 4 41·45

Hygeia.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, July 22 13 48 59·14	21 52 56·49	99 54 22·70
25 13 35 19·57	21 51 4·36	100 0 10·24
29 13 16 55·91	21 48 23·89	100 9 0·18
30 13 12 18·50	21 47 42·28	100 11 22·40
31 13 7 40·08	21 46 59·66	100 13 51·14
Aug. 2 12 58 21·70	21 45 32·86	100 18 57·92
4 12 49 1·71	21 44 4·45	100 24 18·26
5 12 44 20·93	21 43 19·45	100 27 2·08
6 12 39 39·69	21 42 34·01	100 29 51·20
7 12 34 58·36	21 41 48·47	100 32 42·39
8 12 30 16·86	21 41 2·76	100 35 32·02
9 12 25 34·9	21 40 16·63	100 38 26·52
12 12 11 28·21	21 37 57·25	100 47 22·05
14 12 2 3·52	21 36 24·12	100 53 27·41
29 10 51 59·41	21 25 16·83	101 38 46·41
Sept. 2 10 33 38·95	21 22 39·56	101 50 4·05
3 10 29 6·04	21 22 2·46	101 52 43·87
4 10 24 33·74	21 21 25·97	101 55 19·38
5 10 20 2·66	21 20 50·71	101 57 53·45
22 9 5 55·55	21 13 32·83	102 31 59·76
23 9 1 44·13	21 13 17·28	102 33 23·24
29 8 36 59·79	21 12 8·19	102 40 0·40

Themis.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	o ′ ″
1856, Sept. 16 13 50 17·50	1 35 2·51	80 28 34·42
23 13 18 45·40	1 31 0·78	80 49 47·38

Note on the Constancy of Solar Radiation.
By Professor C. Piazzi Smyth.

In this paper the author gives the results of the discussion of a series of earth-thermometer observations, carried on at the Observatory of Edinburgh during the period comprised between the years 1838 and 1854. The thermometers were observed once a-week during this period, and are stated by their author to be admirably adapted for equalising temporary meteorological variations, and for giving good mean results. Their bulbs, filled with alcohol, are buried in the porphyry rock of the Calton Hill, at the several depths of 3, 6, 12, and 24 French feet; and the tubes are long enough to rise to the surface of the ground where their scales are placed, and may be read off to ·01 of a degree of Fahrenheit. This set of thermometers was one of several, which were established in and about Edinburgh, in 1837, for the British Association, under the care of Professor J. D. Forbes. The excellence and the completeness of the burial of the bulb of every thermometer is vouched for by the length of time, which the wave of summer heat is found to occupy in reaching each bulb in succession, according to its depth. Thus the

3-feet thermometer has its maximum in August.
6   —   —   —   September.
12   —   —   —   October.
24   —   —   —   December.

Again, from the annual range increasing with the depth, as,

3-feet thermometer, annual range, $15°$
6   —   —   9·8
12   —   —   4·6
24   —   —   1·2

The following is the mean result for each thermometer during the whole period, extending from 1838 to 1854.

t_4, 3-feet thermometer   .. $46°27$
t_3, 6 —   —   .. $46°55$
t_2, 12 —   —   .. $46°94$
t_1, 24 —   —   .. $47°24$

These results point out to a heated terrestrial centre, even by approaching so small a space as three feet. On the whole, they indicate an increase of $1°$ Fahrenheit for 21 feet of difference of depth.

The author, having eliminated the effects due to the internal heat of the earth, gives the annual results for each thermometer during the period to which the observations refer. He remarks

that if the numbers thus given be projected with the times, the resulting curves contain appearances of periodical waves distributed over a secular swell, with so long a period, that only a small portion of it appears in the period of seventeen years. The observations would, therefore, seem to indicate that the sun may be included in the class of variable stars.

Note on the Occultation of Jupiter by the Moon, August 19, 1856, *observed at Barrie, Simcoe County, Canada West, in West Longitude* 5ʰ 18ᵐ 20ˢ±, *and North Latitude* 44° 25′±. By Charles B. Chalmers, F.R.A.S.

(Communicated by the Rev. W. R. Dawes.)

" At 12ʰ 50ᵐ, local mean time, the moon was approaching the planet, three of whose satellites were visible, the third and fourth being to the west, and the second to the east.

" I lost sight of the three satellites some minutes previous to the occultation, in consequence of the haziness which the atmosphere presented in the neighbourhood of the moon; but the planet was as bright when in apparent contact with our satellite as he was ten minutes before. The planet did not appear elongated or distorted in any way; and the only circumstance worthy of being noted about the immersion was this, viz. it seemed to me (if I may so express myself) as if the moon wished to repel any contact:—in fact, the planet appeared stationary for some seconds, and yet to my eye was perfectly round. This illusion I attribute to refraction; but I should like to have the opinions of more experienced astronomers expressed through the medium of the *Monthly Notices*.

" Shortly after the immersion, the atmosphere became clear, with the exception of a halo round the moon, at a distance of about 12°; that is, the halo was at that distance, and about 2° in breadth. When the two satellites (the third and fourth) reappeared at the dark limb, they were, as well as I could judge, as bright as if the moon had been absent altogether; and the planet at its immersion presented a very beautiful appearance, forming a very fine and elongated crescent, much more so than one would have expected to see; but there was no diminution in its brightness. Clouds intervened before the last satellite (the second) reappeared."

The telescope employed was a very excellent 3½-foot refractor, by Mr. G. Dollond, with a 2¾-inch object-glass: power 60. Time of the phenomena not accurately noted. The belts of *Jupiter* were very distinct, especially the higher or southern one.

Micrometrical Measures of Antares.   By Thomas Maclear, Esq.

(Letter to the Astronomer Royal.)

" I received the other day No. 3, vol. xvi. of the Royal Astronomical Society's *Notices,* wherein you remark that the small elevation of *Antares,* even when on the meridian, and the consequent confusion of image, make it impossible to see it as a double star in your latitudes, &c.

" Here even, when the definition is bad, the *secondary* cannot be distinguished; but in fair definition its relation to the *primary* can be measured with great precision; bearing in mind, with respect to the distance, that they are nearly on the same parallel, therefore, without the aid of a good *driving* clock, and because their magnitudes are very unequal, the distance measurement would be difficult.   I have no reason for complaint against my *driving* clock.

" The measures on the next leaf exhibit a trace of change in the angle and distance since the year 1849.

" Excepting the first series, in the year 1849, each partial set of measures consists of ten, the observations being recorded on the well-known forms of days gone by.   I send the last (No. 232) to show how this binary may be worked here during good definition.

" The last ten measures of $\alpha^1 \alpha^2$ *Centauri* were on May 22d, angle 306°·5, distance 3"·96 (perhaps a little too great).   In fact, the distance has altered little for a long time, but the angular motion is increasing.

Antares.

Angle of Position and Distance of the Components.

Epoch.	Angle.	No. of Obs.	Distance.	No. of Obs.	
1849·640	276°·59	35			*Primary,* orange-red.
1849·663			3·718	20	*Secondary,* deep ultra-marine.
1849·679			3·693	80	
1849·680	276·20	80			
1850·211	275·31	80			
1850·257	275·37	20	3·519	20	
1850·371	273·80	30	3·539	30	
1851·372	274·26	10	3·598	10	
1852·556	273·86	20	3·598	20	
1855·353	273·68	10	3·603	10	
1856·389	273·81	10	(3·282)	10	Indifferent definition.
1856·478	273·48	20	3·453	20	

8¼ feet equatoreal, and power 464 always employed.

Royal Observatory, Cape of Good Hope,
    1856, *June* 18*th.*

. The following are the individual measures in position and distance corresponding to No. 232, which were forwarded by the author,—

Date, 1856, June 10.

Power.	Position.	Power.	Distance Rev. Pts.
	° ′		
464	258 45	464	+ 30·013
	16		·019
	40		·006
	45		·006
	34		·010
	23		———
	26		·054
	16		−30·270
	24		·272
	36		·275
	—		·280

	° ′		·280
Mean	258 30·5		———
Zero for position	+ 15		·377
	———	Mean ⎰	+ 30·0108
	−273 30·5	⎱	−30·2754
	= 273·508		
		Divide by 2	·2646
		Parts =	·1323
		Seconds =	3‴·449

N.B. The plus and minus readings to be taken alternately.

Longitudes derived from Lieutenant Dayman's Observations of the Solar Eclipse of 1854, Nov. 19. By the Rev. R. Main.

(*Letter to Captain Washington, R.N.*)

" A short time ago I forwarded to you the longitude results derived from Lieutenant Dayman's observations of occultations of stars by the moon made on the coast of Africa. I am now able to send you the results for the longitude of Port Elizabeth, Algoa Bay, from his observed times of the beginning and end of the solar eclipse of 1854, November 19.

" Assuming that the diameters of the sun and moon used in the calculations are correct, and that the relative error of the sun and moon's N.P.D. produces an insignificant effect (which is the case in this instance), the longitude east derived from the time of commencement of the eclipse, is

1^h 43^m 6^s·01 + 1·757 × excess of corrections in arc of moon's assumed R.A. above corrections of sun's R.A.

" We have no observations very near the lunar conjunction

of Nov. 19, 1854, to enable us to determine accurately the error of R.A. of the moon; but from observations of other lunations, I have reason to believe the errors to be very considerable, and I think we may assume —25″ to represent pretty well the corrections in arc due to the relative errors in R.A. of the moon and sun.

" The longitude will then be

$$1^h\ 43^m\ 6^s{\cdot}01 - 1^s{\cdot}757 \times 25 = 1^h\ 42^m\ 2^s{\cdot}208\ \text{east.}$$

" Similarly the resulting longitude from the observations of the end of the eclipse is

$$1^h\ 42^m\ 47^s{\cdot}82 - 1^s{\cdot}106 \times 25 = 1^h\ 42^m\ 20^s{\cdot}17\ \text{east.}$$

" *Royal Observatory, Greenwich*, 1856, *July* 15."

A gentleman has for disposal an achromatic refractor, by the elder Tulley, having a clear aperture of $3\frac{1}{4}$ inches, and a focal length of 32 inches. It has a triple object-glass, and is provided with two terrestrial eye-pieces — a large diagonal, with four powers — six celestial eye-pieces, of the respective powers of 40, 60, 84, 132, 232, 288, and a finder. It has vertical and horizontal movements, and is mounted on a solid but very unworthy tripod stand. It performs satisfactorily all the more important tests, and is in perfect order.

The telescope belonging to the late Dr. Miller of Whitehaven is now for sale. It is an achromatic refractor of admirable definition, mounted equatoreally, in the German style, and driven by clock-work. The aperture of the object-glass is $4\frac{1}{10}$ inches and the focal length 69 inches. There is an ample supply of eye-pieces and coloured glasses, and a parallel wire-micrometer of beautiful workmanship, by Simms, with powers ranging from 50 to 500.

The telescope is by Cooke of York and is nearly new. It will divide ζ *Boötis*, ζ *Herculis*, λ *Ophiuchi*, &c. Further particulars, price, &c., may be known by application to Isaac Fletcher, Esq., F.R.S., Tarn Bank, Workington.

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ERRATA, VOL. XVI.

Page 152, line 18, *for* objections, *read* observations.
— 152, — 30, *for* n^2 *Andromedæ, read* γ^2 *Andromedæ.*
— 173, *Lætitia,* April 19, App. N.P.D., *for* 78° 49′ 47″·13, *read* 77° 49′ 45‴·31.
— 185, line 13 from top, *for* zinc, *read* time.

LIST OF PRESENTS

RECEIVED DURING THE SESSION OF 1855-56,

AND OF

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APPENDIX VII.

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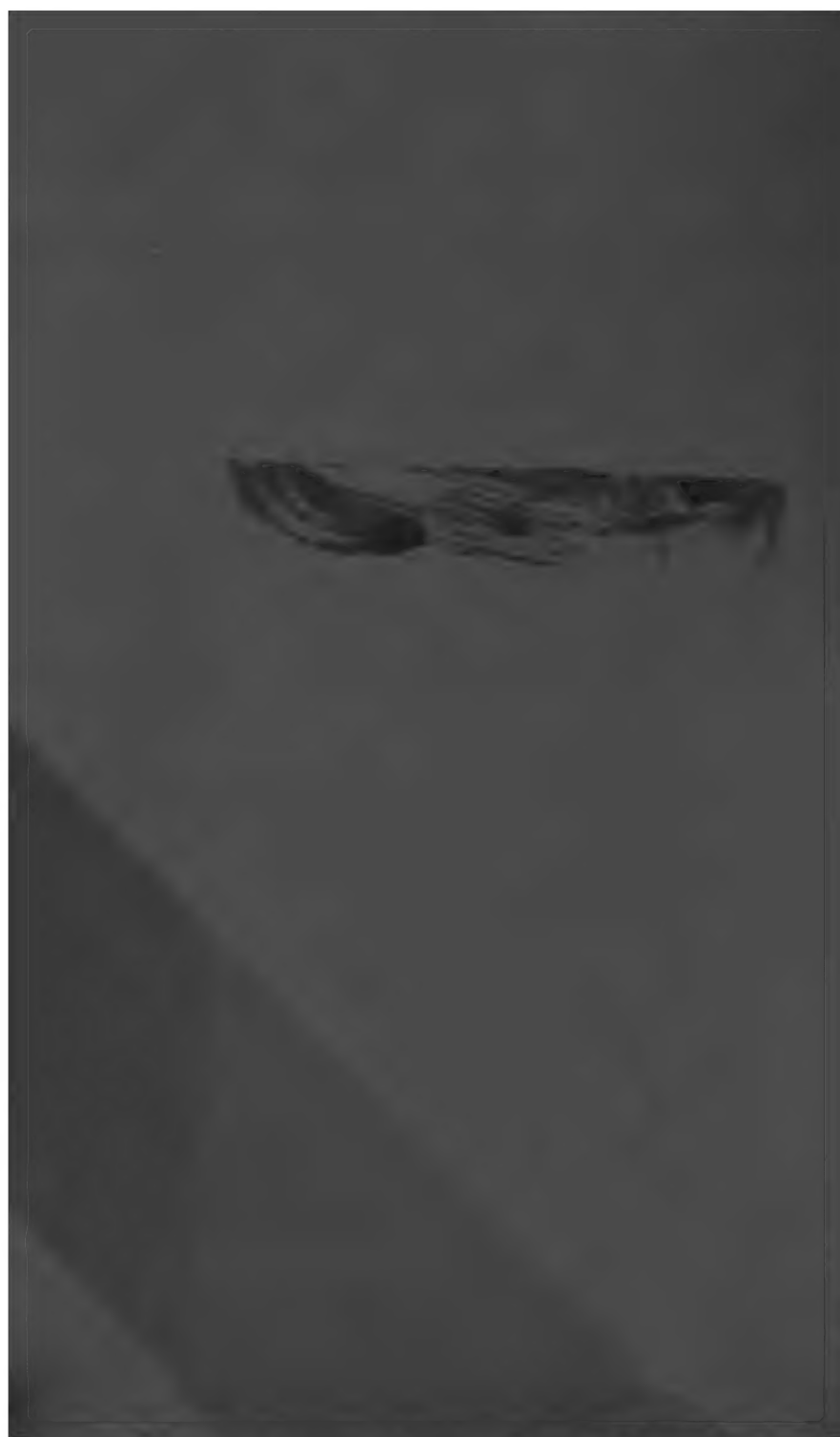nomicæ Tabulæ, folio.   *Paris,* 1545